The Crime Control Effects of Criminal Sanctions for Intimate Partner Violence

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Joel H. Garner
and
Christopher D. Maxwell

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Abstract

This research assesses the extent to which criminal sanctions–prosecution, conviction, and jail–are imposed for violence between intimate partners and whether those sanctions are associated with repeat offending. A review of 135 English language publications reporting on criminal sanctions for violence between intimate partners and determined that one third of all reported offenses and about three fifths of all arrests for intimate partner violence result in a prosecution. This research also found that one in six reported offenses, one third of all arrests, and over one-half of all prosecutions for intimate partner violence result in a conviction for intimate partner violence. A detailed review of 32 studies found that the predominant finding reported in this literature is that criminal sanctions have no effect on repeat offending. This review also found that methodological weaknesses–small sample sizes, diverse measurement of sanctions and of repeat offending, and the absence of statistical power analyses–limit the ability of these studies to provide a firm basis to test theory or evaluate public policy. Secondary analyses of the data available from these studies determined that the use of more consistent methods and measures across 12 sites generates the same general conclusion–criminal sanctions are not significantly associated with less repeat offending. These secondary analyses are limited to the use of prevalence measures from official records.

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Chapter 1:

The Amount of Prosecution and Conviction for Intimate Partner Violence Reported in the Published Literature

The advent of the modern domestic violence reform movement focused attention on the nature and extent of arrest and criminal prosecution for intimate partner violence (Fagan, 1996; Pleck, 1987; Worden, 2000; Zorza, 1992). Domestic violence scholars have criticized law enforcement agencies for their failure to report domestic violence offenses, to arrest male offenders, to redress the victimization of women, and to treat violence against intimate partners as severely as violence against non-intimate partners (Blari, 1979; Dobash & Dobash, 1979; Fleming, 1979; Martin, 1976; Miller, 1970; Paterson, 1979; Roy, 1977; Walker, 1979).

With the widespread adoption of pro-arrest laws and policies in the 1980s, the general improvements in the relationships between law enforcement agencies and reform advocates, and the appearance of research which showed deterrent effects for arrest (i.e., Maxwell, Garner & Fagan, 2002; Sherman & Berk, 1984), the policies and practices of prosecutors and judges toward intimate partner violence have attracted heightened attention (Ford & Breall, 2003). Despite this increased attention and focus, the use of criminal sanctions beyond arrest to deter and incapacitate intimate violence offenders remains controversial (Fagan, 1996; Mills, 1998; Mills, 2003; Sherman, 2000; Stark, 2004). Of special concern are two alternative views of prosecution. Is prosecution is a resource that victims can choose to invoke or not invoke as they see fit or should prosecution be the mandated response to violent behavior between intimate partners (Ford, 1991)?

These are not merely academic disputes. Victims of intimate partner violence frequently mobilize the criminal law by calling the police (Felson & Pare, 2005) or by filing a complaint with

local prosecutors (Ford, 1983; Schmidt & Steury, 1989) with the expectation that formal legal actions or the opportunity to choose formal sanctions will protect them, their children, and their personal property from future harm. Legitimate concerns about the failure of the criminal justice system to meet those expectations are frequently punctuated by notorious examples of the assault, rape, injury, and murder following inadequate and, in some cases, illegal failures of the police, prosecutors, and the courts to protect victims from their intimate partners (e.g., Gee, 1983; Martin, 1976). The political and the scholarly controversy centers around the extent to which these real failures are infrequent anomalies, common but not typical responses, or whether they reflect a systematic failure of the entire system of criminal justice.

This research seeks to contribute to discussions about this issue by focusing on two important but primarily empirical questions. First, how often do reported incidents of intimate partner violence result in a prosecution and, second, how often do those prosecutions result in a conviction? The answers to these two questions will not, in themselves, resolve ongoing controversies about the appropriateness of relying on the criminal law to protect victims of intimate partner violence, but the answers may inform that debate, eliminate unnecessary disputes, and speed the adoption of more evidence-based public policies that reduce violence against women.

Conventional Wisdom about Prosecution and Conviction Rates

Our review of the scholarly literature finds nearly unanimous assessments that the prosecution and conviction for domestic violence offenses occur rarely. Based on data collected during the 1950s and 1960s, Parnas (1967; 1969; 1970; 1971) consistently emphasized the paucity of response to domestic violence by the criminal courts. Based on the same field research as Parnas, Miller's (1970, p. 266) treatise on prosecution reaches a similar conclusion: the number

of "family and neighborhood disputes is vast, yet very few of these complaints ever result in prosecution." Martin's (1976, p. 110) assessment is that "standards set by prosecutors for accepting a case for trial are so restrictive that wife-abuse cases rarely qualify." By the end of the 1970s, Dobash and Dobash (1979, pp. 217-18) assert that

> (T)he judicial response to violence against wives generally reflects the same pattern of indifference, official inaction, and occasional unofficial reaction exhibited by police departments.

Walker (1979, p. 212) viewed immediate prosecution as a potent technique to halt violence against women that, "is rarely used." Lerman's (1981, p. 8) states that "at present, most battered women do not, in fact, have the option to file charges, because the obstacles posed by the system are so great" and that "in most places prosecution is seldom an available remedy for battered women (p. 29)." Buzawa and Buzawa (1990, p. 64) claim that low rates of prosecution and conviction re-enforce the persistent reluctance of police officers to become involved in domestic violence cases. Sherman (1992, p. 244) contends that there is "widespread under prosecution of domestic violence cases." Tolman and Weisz (1995, p. 483) report that "prosecution rates for domestic violence cases have been typically low."

These negative assessments are also reflected in two reports from the National Academy of Sciences. Crowell and Burgess (1996, p. 118) report that "prosecution rates of battering cases typically have been low." Two years later Chalk and King (1998, p. 279) state that

> (T)he criminal justice system has traditionally been reluctant to impose fines, sentences, and other punitive sanctions on individuals charged with child maltreatment, domestic violence, or elder abuse.

Sherman (2000, p. 263) asserts that "domestic violence arrests in big cities are rarely followed by prosecutions," while Worden's (2001, p. 221) review states that historically "police policies of disengagement resulted in extremely infrequent prosecutions and adjudications." Hartman and

Belknap (2003, p. 351) report that "although proarrest policies resulted in unprecedented numbers of batterers referred to the criminal court, serious prosecutions of these cases may still be unlikely." Stroshine and Robinson (2003, pp. 100-101) assert that "arrest for domestic violence is a rare event" and that "(P)rosecution is even more infrequent." Finally, Jordan (2004, p. 1420) reports that "victims of domestic violence face low rates of prosecution."

We could find only three reports that questioned the assertion that the prosecution or conviction for intimate partner violence is rare or infrequent. In a synthesis of data from four domestic violence studies from the early 1980s, Dutton (1987: 199) reports that there were 910 convictions (53.1%) out of 1,713 arrests. Elliott (1989, pp. 459-462) notes many claims that prosecutors rarely file charges in domestic violence cases referred to them by the police, but found little empirical evidence to support or refute those claims. More recently, Klein's (2004, p. 134) review of more than a dozen studies published between 1996 and 2002 concludes that "jurisdictions vary widely in their filing and prosecution rates of domestic violence crimes." Thus, Dutton finds conviction rates greater than 50%, Elliott is agnostic, and Klein asserts that prosecution and conviction rates vary from jurisdiction to jurisdiction.

Except for Dutton (1987), these reviews lack precision. They often fail to define what constitutes a prosecution or a conviction. They frequently employ vague terms like "low," "rare," or "infrequent," rather than reporting quantitative indicators that are juxtaposed against an expected or standard prosecution or conviction rate. In fact, there are no accepted standards for what would constitute an appropriate amount of prosecution or conviction for any offense (Cole, 1993; Commission on Trial Court Performance Standards, 1997), let alone for violence between intimate partners (Mills, 2003; Parnas, 1969; Parnas, 1971; Zorza, 1992). Similarly, at the present time, our theories of justice, deterrence, and incapacitation, not to mention our

understanding of the etiology and the dynamics of intimate partner violence, are not sufficiently developed to specify what types and amounts of criminal sanctions are either necessary or sufficient.

There are other impediments to establishing appropriate rates of prosecution and conviction. As Jacoby (1975) and Davis, Hamilton, and Weisburd (2007) discuss, there are complex interactions among individual performance measures for prosecutors. For instance, if only the most serious offenses, like homicide, are reported to the police, arrest rates, prosecution rates and conviction rates are likely to be high. If the severity of offenses reported to the police varies and the police make an arrest in every incident, a prosecutor charging only the most serious offenses would have a low prosecution per arrest rate; however, that same prosecutor may have a high conviction per prosecution rate for the few prosecutions that are initiated. Thus, in any particular jurisdiction or any group of jurisdictions, examining individual performance measures alone is unlikely to capture the true performance of the criminal justice system.

Explaining Rates of Prosecution and Conviction

The lack of prosecution and conviction have been linked to the attitudes of society and of the actors within the criminal justice systems who are indifferent to or perhaps even supportive of violence against women (Dobash & Dobash, 1979; Martin, 1976). Parnas (1967) and Miller (1970) present a different perspective. While both seem genuinely appalled at the nature of intimate partner violence, they question whether U.S. urban court systems of the 1950's were capable of increasing the amount of criminal sanctions and, even if they could, whether any such increase would reduce the amount of intimate violence. Zimring (1989) questions the extent to which the criminal law is ever likely to be an effective mechanism to address intimate partner violence. All of these perspectives have limited explanatory power since they report no variation

in prosecution or conviction rates and no variation in prevailing attitudes or the capabilities of the criminal justice system. On the other hand, Klein (2004, p. 134) states that "in many jurisdictions, prosecutions have increased markedly," suggesting general improvement over the six year range in his studies: however, he does not present any pre and post data for a particular jurisdiction or correlate prosecution or conviction rates with the timing of these research studies to support this view.

Implications of the Assertions About Low Rates of Prosecution and Conviction

The widely accepted conclusion that prosecution and conviction rates are low has been used to substantiate general criticisms of current criminal justice system operations, as well as detailed criticisms about how specific criminal justice officials and agencies respond to intimate partner violence. Some critics (e.g., Zorza, 1994) have argued that it is not reasonable to expect arrest to be an effective deterrent for domestic violence, given that prosecution and conviction for this offense are so rare. Others suggest that the low probability of prosecution and conviction preclude these types of sanctions from being useful in their own right as a general prevention for repeat victimization. In addition, the presumption of low prosecution and conviction rates has been used to both support and to dispute the argument that mobilization of the criminal law is bad for victims (Mills, 2003; Stark, 2004).

Questioning Conventional Wisdom

Despite the prevailing consensus that both prosecution and conviction for intimate partner violence are rare, we sought to identify, document, and summarize all the existing studies that report the amount of prosecution or conviction for intimate partner violence. We began this review expecting to find a handful of studies from which a single reasonably reliable estimate

could be produced. We were wrong on several counts. First, we identified, not a handful, but 135 reports from which it is possible to calculate either a rate of prosecution or a rate of conviction for intimate partner violence. The existence of such a substantial literature is, we think, an important finding in itself and one that suggests that a large body of research has not been incorporated into previous assessments. Second, given the diversity in definitions, measures, methods, and sample sizes used in this research literature, a simple statement about a single, average rate of prosecution or conviction is likely to be misleading. Third, the variability in prosecution and conviction rates complicates description using simple measures.

Methods

Identifying Studies to Review

We initially sought out English language reports of the prosecution and conviction for intimate partner violence regardless of the time period or jurisdiction studied. We conducted extensive automated and hand searches of the prior research. We examined references in known articles and utilized web based searches through the National Criminal Justice Reference Service, Google search engines, and internet-based reference services to identify, obtain, and review hundreds of documents, published articles and books. When more than one report of the same sample was available, we choose the largest, most inclusive and most detailed of the reports to include in this review. Sometimes, a single document included separate analyses of different samples of offenses, arrests, or prosecutions and we included these analyses as separate reports. We included analyses that provided sufficiently clear and consistent information to determine the

number of offenses, arrests[1], prosecutions, and convictions needed to compute prosecution or conviction rates.

The relationship between the victim and offender in all of the cases included here involves either intimate partners or other family members. In most studies, the relationship is exclusively intimate partners and, in each study, the predominant relationship is one of intimate partners. The studies we identified typically involve misdemeanor assault but some studies include a mix of offense types from verbal abuse to homicide. Although most studies and cases involve a sole male offender, we accepted studies with female offenders and with dual arrests. Our sample accepts heterogeneity in offense types, victim–offender relationships, and offender sex because the research literature referenced above make none of these distinctions when asserting that prosecution and conviction are infrequent.

[1] We count summons issued or warrants served as an arrest.

We excluded general studies of criminal case processing (e.g., Albonetti, 1986) unless the necessary information on intimate partner violence could be easily extracted from the printed reports (e.g., Vera Institute of Justice, 1977; Forst, Lucianovic & Cox, 1977). Because of the high proportion of non-intimate offenders, reports of the prosecution and conviction for sexual assault are not included here (Daly & Bouhours, 2008). Many valuable reports about the prosecution of intimate partner violence were eventually eliminated because they did not include sufficiently complete and consistent quantitative information to produce a prosecution or a conviction rate for a particular jurisdiction at a known time period (e.g., Parnas, 1969; Berk, Rauma, Loseke & Berk, 1982; Lyon & Mace, 1991). For these reasons, the reports included here are the appropriate sample to assess the amount of prosecution and conviction for intimate partner violence.

Study Characteristics

The 135 reports we selected use data from over 170, mostly urban, jurisdictions located in Australia, Canada, Switzerland, the United Kingdom, and 25 states in the U.S. Most of the 135 reports included in this review are based on official data of individual cases from law enforcement agencies, prosecutor files, or court records. A dozen reports included in the following analyses are based on data derived from victim interviews; five of the 12 reports are based on a probability sample of adults in the United States (Felson & Pare, 2007; Tjaden & Thoennes, 2000), in Kentucky (Schulman, 1979), in the United Kingdom (Walby & Allen, 2004) and in Australia (Mouzos & Makkai, 2004). Although we attribute the findings from these five studies to the entire state or nation surveyed, in this study, we use the unweighted number of incidents and do not use their probability sampling as a basis to extrapolate to national numbers of prosecutions or convictions. Seven interview-based reports (e.g., Belknap & Sullivan, 2003; Fagan, 1989; Finn, 2003; Fleury, 2002; Keilitz, Jones & Ostrom, 1999; Marsland, Plecas & Segger, 2001; Steketee,

Levey & Keilitz, 2000) are limited to local jurisdictions. Despite their obvious strengths, victim surveys must rely on the victim's knowledge and memory of the offender's case processing at the time they are interviewed.

All of the reports based on victim interviews and 114 of the reports based on official records track the disposition of individual intimate partner violence offenses or arrests to the decision to file charges or individual offenses, arrests and prosecutions to a guilty or not guilty verdict. These studies commonly have missing data on case dispositions. We have included all cases sampled by these studies in our calculation of rates and counted cases with missing data as not prosecuted or not convicted. Cases recorded as pending or *nolle prosequi* are defined as not convicted. Although this approach likely underestimates the actual prosecution and conviction rates, it provides a consistent basis for computing rates across studies.

Another group of nine studies identified here are statistical reports prepared by state or local agencies or advocacy organizations (e.g., Ransbottom, Stein & Libertun, 2006; Toon, Hart, Welch, Coronado & Hunting, 2005; Turley & Haas, 2004; Woolery, 2004). These studies do not prospectively tract selected cases to disposition but provide snapshots of the number of arrests, prosecutions, and convictions in a particular jurisdiction or group of jurisdictions in a given time period. These retrospective samples do not appear to have the same missing data problems as prospective research samples because these statistical reports typically do not follow individual cases to completion. With large samples and some reasonable assumptions about the stability of prosecution and conviction rates in a particular jurisdiction over time, the estimates they provide can be comparable to the rates generated by tracking research samples. For this reason, we have included both prospective tracking and statistical snapshots studies in our review.

Defining Prosecution and Conviction Rates

For purposes of this study, we define a prosecution as any incident where one or more criminal charges are filed in response to a report of intimate partner violence. We count police laying of charges in Canada and the U. K. as equivalent to filing of charges by prosecutors in other jurisdictions (Walsh & Poole, 1983). Our definition of conviction similarly relies on a formal criminal justice event. We count only formal convictions. Deferred prosecution or treatment programs are not counted as a conviction unless they are explicitly part of a sentence following conviction. This definition of conviction seems most relevant to recent developments in the jurisprudence of violence against women, such as the restrictions on gun possession for a domestic violence conviction authorized in the Violence Against Women Act (18 U.S.C. § 922(g)(9)). Our definitions identify two easily measured events and sharply distinguish between the initiation of a prosecution and the completion of a prosecution as a conviction. We apply our explicit definitions to a diverse body of research and do not necessarily employ the same definitions of prosecution and conviction used in the original reports. We applied these definitions[2] to record the number of offenses, arrests, prosecution and convictions included in each report.

We constructed two prosecution rates–prosecutions per reported offense and prosecutions per arrest–and three conviction rates–convictions per reported offense, convictions per arrest, and convictions per prosecution. Prior statements about the infrequency of prosecution and conviction do not consider that there are multiple types of prosecution and conviction rates that might provide different assessments of the amount of prosecution in different jurisdictions.

Results

[2] The relevant data elements were initially identified and coded by the first author. The codes were then revised, if necessary, after a review by the second author and a research assistant. The data used in the study are available from the National Archive of Criminal Justice Data.

We summarize our basic findings in tables 1 through 4 and provide detailed listings of the reports used to compute each rate in appendices 1 through 5. Table 1 includes the number of reports and the number of offenses, arrests, prosecutions or convictions used to calculate each rate. For instance, in Table 1, the rate of prosecutions per reported offense is based on 43 reports that involve 356,352 offenses. The rate of convictions per reported offense is based on 24 reports and less than 10 percent of the number of offenses (n=25,215). The rate of prosecutions per arrest is based on 53 reports involving 94,781 arrests; the rate of conviction per arrest is based on 50 reports involving 84,736 arrests. Lastly, the rate of convictions per prosecution is based on 100 reports involving 268,159 prosecutions.

Within each of these samples, Table 1 reports the average rates of prosecution and convictions based on the total number of incidents included in each sample. However, the approach used in Table 1 can create certain anomalies. For instance, based on the reports and incidents included in Table 1, we compute that 72.6 percent of the reported offenses result in a prosecution but that 57.6 percent of arrests result in a prosecution. This anomaly stems from two characteristics of our samples of reports. First, the 135 reports we identified are based on samples that vary from less than 10 to over 200,000 incidents. Second, our prosecution and conviction rates are based on different samples of reports from different jurisdictions at different points in time. We address the first issue – varying sample sizes – in two ways. First, we compute revised estimates of the rate of prosecutions per reported offense and the rate of convictions per prosecution by excluding certain outliers – reports with very large samples and either very high or very low prosecution or conviction rates. Second, we compute the five prosecution and conviction rates weighing each report equally, regardless of the number of cases.

In Table 2, we compute the rate of prosecutions per reported offense by excluding three

Canadian reports (Ogrodnik, 2006; Patterson, 2003; Trainor, Lambert & Dauvergne, 2002) which included 277,206 (77.8%) of the 356,352 reported offenses in Table 1; the prosecution rate for these three large studies averages 83.2 percent (See the appendix 1 for the details of these reports). The revised rate for prosecutions per reported offense in Table 2 is 35.5 percent, instead of 72.6 percent in Table 1. Outliers[3] affect two other rates reported in Table 1. The rate of convictions per reported offense is derived from a sample that includes two relatively large reports (Kelley & O'Brien, 1994; Cook, et al., 2004) with 8,785 (25.8%) the 34,000 offenses and with conviction rates that average only 0.9 percent (See Appendix 3). Excluding these two reports results in an average rate of 16.4 percent. Similarly, the rate of convictions per prosecution in Table 2 is 47.8 percent after on excluding one report (Maryland Network Against Domestic Violence, 2003) that has 123,507 (46.1%) of the prosecutions in Table 1 and a conviction rate below 21 percent. The revised conviction rate per prosecution in Table 2 is 47.8 percent, compared with 35.3 percent in Table 1 (See Appendix 5). We identified no outliers in the computation of the rate of prosecutions per arrest or convictions per arrest (See Appendices 2 and 4).

[3]We examined each distribution for reports with extremely low or high rates and with large sample sizes. We identified as outliers those reports whose exclusion created a substantial change in the average prosecution or conviction rate.

In Table 3, we address the issue of varying sample sizes in a different way. We compute average prosecution and conviction rates weighing each report equally. This approach retains all 135 reports but considers studies with 10 cases the same as studies with 200,000 cases. The 43 studies for which we could calculate a rate of prosecutions per offense varied from 2.7 percent (Field & Field, 1973) to 84 percent (Ogrodnik, 2006); the mean rate among all 43 studies is 34.4 percent; the median is 27.4 percent. The rate of prosecutions per arrest is 59.2 percent with a low of 4.6 percent (Sherman, 1992) and a high of 98.0 percent (Friday, Lord, Exum & Hartman, 2006). The full range of prosecution rates are displayed in Appendices 1 and 2.

Conviction rates summarized in Table 3 show a similar pattern of great diversity (See Appendices 3, 4 and 5). In 24 reports, the mean rate of convictions per reported offense is 16.4 percent, with a median of 14.5 percent.[4] This rates ranges from 4.0 percent (Steketee. et al., 2000) to 53.1 percent (Urbis Keys Young, 2001). The average rate for convictions per arrest is 30.5 and the median of 32.0 percent. These conviction rates range from 0.4 percent (Sherman, 1992) to 65.0 percent (Dunford, et al., 1990). The rate of conviction per prosecution is captured in 100 out of 135 reports; it has a mean of 51.2 percent, a median of 50.2 percent, a minimum of 8.1 percent (Sherman, 1992) and a maximum of 98.9 percent (Salazar, Emshoff, Baker & Crowley, 2007). Both approaches to addressing the issue of outliers (Tables 2 and 3) generate similar rates – 35.5 and 34.4 percent, 57.6 and 59.2 percent, 16.4 and 16.4 percent, 30.5 and 32.0 percent, and 47.8 and

[4] Tests for skewness and kurtosis of the rates in Table 3 show that only one distribution (convictions per reported offense) violates assumptions of normality. When one report (Urbis Keys Young, 2001) with a high conviction rate is excluded, that distribution conforms to normality and the average rate drops from 16.8 to 15.0 percent.

51.2 percent.

Tables 2 and 3 do not address the concern that our five rates are derived from different samples of reports. Seventeen of our 135 reports (10.4%) have complete data on all four stages of criminal case processing use here--reported offenses, arrests, prosecutions and convictions. From these 17 reports we can construct all five prosecution and conviction rates. The rates derived from these 17 reports are presented in Table 4. These rates are based on a relatively small number of reported offenses (n=22,030), arrests (n=7,651), prosecutions (n=3,797) and convictions (n=2,252); however, the rates derived from this sample are similar to the rates reported in Table 3. The two prosecution rates in the smaller sample are 27.1 and 54.6 percent compared to 34.4 and 59.2 percent in the larger sample in Table 3. The three conviction rates are 15.8, 30.9, and 55.7 percent in the smaller sample and 16.4, 32.0 and 51.2 percent in the larger sample. The similarities between the prosecution and conviction rates in Tables 3 and 4 diminish but do not eliminate the concern that the average rates in Table 3 are derived from a different sample of reports drawn from different jurisdictions at different points in time.

Our assessment of average prosecution and conviction rates for intimate partner violence is based on all known reports and incorporates corrections for outliers and variability in nature and number of reports included in the calculation of each rate. It would be preferable to have a large, representative sample of jurisdictions for which complete information is available about criminal case processing. However, given the nature of the reports available and the consistency in findings in Tables 2, 3 and 4, we rely on the results of Table 3 to estimate average rates of prosecution and conviction for intimate partner violence.

Based the available data from the 135 reports summarized in Table 3, we estimate that one third of all reported offenses and about three fifths of all arrests for intimate partner violence result

in a prosecution. We also estimate that one in six reported offenses, one third of all arrests, and over one half of all prosecutions result in a conviction for intimate partner violence. One interpretation of the qualitative assertions in the prior review literature is that prosecution and conviction rates are *consistently* rare or infrequent. This interpretation is not supported by any of our findings. All five prosecution and conviction rates (including the conviction per offense) vary greatly from one jurisdiction to another. It is clear from our findings that prosecution and conviction do occur infrequently in some jurisdictions at some points in time but, as a general statement about the operation of the criminal justice system, such blanket assertions do not conform to the results of our systematic review of 135 reports.

Have Prosecution and Conviction Rates Changed Over Time?

As late as 1980, the official position of the normally progressive American Bar Association was to discourage arrest and prosecution for intimate partner violence. Their standard 1-3.4 stated

> (T)here should be clarification of the authority of police to use methods other than arrest and prosecution to deal with the variety of behavioral and social problems which they confront

and the police should

> engage in the resolution of conflict such as that which occurs so frequently between husband and wife or neighbor and neighbor in the highly populated sections of the large city, without reliance upon criminal assault or disorderly conduct statutes (ABA,1980, p. 1.76).

By the mid-1990's, the conventional wisdom in the United States had changed. The 1994 Violence Against Women Act and its re-authorizations strongly promoted the use of prosecution as the appropriate response to intimate partner violence and, by 1996, a large proportion of urban U.S. prosecution offices had adopted a variety of hard and soft “no-drop” prosecution polices for

intimate partner violence (O'Sullivan, Davis, Farole, & Rempel, 2007; Rebovich, 1996). Moreover, a highly publicized effort in the U.S. promoted the systematic collection of crime scene evidence for increasing the successful prosecution of intimate partner violence (Gwinn & O'Dell, 1993).

We examine our data to assess the extent to which these policy changes are reflected in reported rates of prosecution and conviction for intimate partner violence based on the methods used in Table 3. For each report in our research, we recorded the year when data collection began. For studies in the United States, the correlations of year data collected with both prosecution rates are positive, that is, they show improvements over time (See Table 5). The correlations of the year data collection began with two of the three conviction rates are negative, suggesting deterioration over time. At .041, the one positive correlation is small. We also examined the nature of changes in reports with data collected before or after the passage of the 1994 Violence against Women Act. The average rates in reports of prosecuting intimate partner violence in the United States are 12.7 percent and 26.6 percent greater after 1994 (See Table 5) . In addition, two of the three conviction rates for reports whose data were collected after 1994 are 15.3 percent and 3.0 percent greater than the reports with data collected before 1995. However, the rate of convictions per reported offense after 1994 is 17.8 percent lower than the rate before 1995. These findings provide some support for the notion that prosecution and conviction rates for intimate partner violence have increased over time in the United States but that assessment is conditioned by the relative importance of different prosecution and conviction rates.

U.S. vs. Non-U.S. Jurisdictions

Table 6 describes the five prosecution rates for the 95 reports from U.S. jurisdictions and

the 40 reports from jurisdictions in other countries.[5] The average prosecution and conviction rates show differences between reports for U.S. jurisdictions and for jurisdictions outside the U.S. However, there is no consistent direction for these differences. The rate of prosecution per offense is lower in the U.S. (27.6%) than in other countries (45.5%) but the prosecution per arrest rate is higher in the U.S. (61.6%) than in other countries (50.7%). Similarly, the conviction per reported offense and the conviction per prosecution rates (14.4% and 49.9%) are lower in the U.S. than in the other countries in this review (17.6% and 53.8%). The rate of convictions per arrest, however, is higher in the U.S. (33.0% versus 25.6%). Thus, comparative assessments across nations also depend on which measure of prosecution or conviction is used.

The averages for all the U.S. based reports are similar to the averages generated by the two U.S. based national probability samples. At 24.7 percent, the prosecution per offense rate generated from Tjaden and Thoennes's (2000) national probability sample for the U.S. is similar to the median rate reported in Table 3 (27.4%) but lower than the mean for all U.S. based reports (34.4%). The average conviction per offense rate for U.S. studies is 16.4 percent; Tjaden and Thoennes report 10.5 percent and Felson and Pare (2007) report 18.2 percent. The average conviction per arrest rate for U.S. reports is 32.0 percent; the national probability samples report 32.0 percent and 37.7 percent respectively. The average conviction per prosecution rate for U.S. studies is 51.2 percent: the rate Tjaden and Thoennes report is 41.6 percent.[6]

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[5] The non-U.S. reports are from the Canada (n=19), United Kingdom (n=16) Australia (n=5) and Switzerland (n=1). The non-U.S. averages in Table 6 are based on as few as five reports.

[6] Felson and Pare (2007) do not report arrests; therefore, we cannot generate prosecution or conviction rates per arrest for that

study.

More than two thirds (69.6%) of the 135 reports are from U.S. jurisdictions. The rates reported for all jurisdictions are similar to those for the United States, except for the rate of prosecutions per reported offense. Among all reports, approximately one in three reported offenses result in a prosecution. Among U.S. reports, the rate is closer to one in four reported offenses resulting in a prosecution.

Discussion

Our research has numerous strengths over prior approaches at summarizing the nature and extent of prosecution for intimate partner violence (e.g., Elliott, 1989; Klein, 2004; Chalk & King, 1998). We present data from a larger and more diverse set of reports. We employ explicit definitions and measures of prosecution and conviction. We describe and analyze multiple quantitative measures of the reported amounts of prosecution and conviction for intimate partner violence. We provide some of the systematic empirical evidence that Elliott (1989) found missing and which subsequent reviews have failed to incorporate in their assessments (e.g., Crowell & Burgess, 1996; Worden, 2000).

Overall, based on the 135 reports from over 170 jurisdictions in five countries, approximately one third of intimate partner violence offenses reported to the police result in a prosecution and that three fifths of arrests result in charges being filed. Moreover, about one third of the arrests and over half the prosecutions result in a conviction. These findings are not quite as high as Dutton's (1987) 53.1 percent conviction rate but they support Klein's (2004) assertion that prosecution rates vary across jurisdictions. Our findings are not consistent with reports from the National Academy of Sciences (Crowell & Burgess, 1996; Chalk & King, 1998), Sherman (1992; 2000), Buzawa and Buzawa (1990), and Hartman and Belknap (2003) that prosecution rates for intimate partner violence are rare, infrequent, or typically low. Our conclusion is that when

intimate partner violence is reported to the police or to prosecutors, local criminal justice systems produce highly variable prosecution and conviction rates and that, on average, these rates are notably more substantial than most previous reviews have recognized.

Limitations of this research

There are several limitations to our conclusions regarding the amount of prosecution and conviction. First, the 16.4% rate of convictions per reported offense could be interpreted as support for or, at least, not a clear rejection of an assertion that reported offenses rarely or infrequently result in a conviction. Thus, while four out of the five rates we compute show average rates that cannot be easily interpreted as rare or infrequent, our general findings are dependent upon how the rate of conviction is defined.

This research has not addressed the common assertion that intimate partner violence is not treated like any other crime (Fagan, 1996). Implicit in that argument is that the level of prosecution and conviction that exists for violence between individuals who are not intimate partners is the level of prosecution and conviction that is appropriate for intimate partners. Of course, the level of prosecution and conviction for violence between people who are not intimate partners may also be inadequate to meet the needs of justice or to produce a crime control effect. Unfortunately, comparing prosecution and conviction rates for intimate and non-intimate partners cannot adequately be addressed by our sample of studies; however, the substantial rates of prosecution and conviction reported here and recent evidence from Durose, et al. (2005:49) suggest that the empirical basis for this common assertion warrants a thorough reexamination.

While informative, the variability in the average prosecution and conviction rates over time and between U.S. and non-U.S. jurisdiction does not conflict with our main conclusions - prosecution and conviction rates vary among jurisdictions and the average rates cannot reasonably

be described as rare, infrequent, or typically low in any time period or country included in this sample. We interpret our cross-national and temporal findings as no more than interesting and a reasonable basis to generate more detailed hypotheses about how political, legal and practical changes might affect a variety of prosecution and conviction rates. The available data are not well suited to test hypotheses about changes over time or between countries- our samples are small and unrepresentative, our analyses are cross-sectional and bi-variate, and our measures are comparable but not uniform across sites. In addition, a more rigorous design would specify, prior to data collection, which aspects of the legal process, beyond simple rate counts, should change over time and include detailed measures of the changes in the criminal justice system that actually occurred in a particular jurisdiction over time.

Similarly, this study was not designed to explain variations in prosecution and conviction rates across U.S. jurisdictions. In addition, there are insufficiently comparable data in the selected reports to conduct such a study. Few of these reports provide any information about the kinds of individual, organizational, and community factors that might be useful in predicting prosecution or conviction rates–resources of the prosecutor, the volume of other crime problems in the jurisdiction, pro-arrest and pro-prosecution policies, local, state, and national laws and cultures, severity of the offense, role of victim advocates, strictness with which firearm provisions are enforced, provisions for defense counsel, etc. Future research that prospectively captures systematic information on these and other possible explanatory factors might be able to provide multivariate and multi-level tests explaining sources of variation but this research cannot.

There are three major methodological caveats to our findings about the amount of prosecution and conviction – measurement, sampling, and variability in findings; despite these caveats, the strength of our design is still sufficient to reject the assertions reported in much of the

contemporary research and reform literature that prosecution and conviction for intimate partner violence are consistently low, rare, or infrequent.

The criminal justice system often acts informally. Charges may not be sought or convictions obtained when an arrested offender's probation or parole are revoked (Kingsnorth, MacIntosh & Sutherland, 2002). Sometimes charges and convictions are expunged (Hirschel & Hutchison, 2001). One common disposition reported in the 135 studies in this review is "deferred prosecution" or "diversion" which we count as a prosecution but not a conviction. Typically, this disposition means that the courts will dismiss the charges after a fixed period of time, such as six months, if the offender is not arrested again (Klein, 2004; Lyon & Mace, 1991). In addition, multiple offenses are occasionally combined into one court disposition.

In many studies, it is not clear to what extent the "deferred prosecution" failures have already been removed and the deferred prosecution disposition means only those given this option who did not re-offend by the time the research data were collected. Lastly, some defendants, like Ken Lay of Enron fame, die before their prosecution is completed and the sentences imposed. Death rates for criminal offenders, while low, are higher than the general population and this can affect the computation of prosecution and conviction rates for intimate partners as well as other offenders (Weisburd, Waring & Chayet, 2001; Zeisel, 1981).

There are other measurement issues. We have used crude dichotomous measures of a complex, lengthy, and dynamic process (Garner, 2005; Robinson, 2003). We do not argue that these definitions are the only appropriate definitions. In many ways, the measures used here are simplistic and do not capture many of the potentially important nuances of the extent or severity of prosecution or tap the meaning of filing charges for defendants or for victims. In this review, we do not consider the number of charges filed, the severity of offenses charged, time to disposition,

or changes in the severity of charges from arrest to prosecution to conviction. Each of these elements can be conceived as components of a more comprehensive measure of the amount or extent of prosecution. All of these concerns are legitimate considerations in a fully developed understanding of the meaning of the amount of prosecution; few of these considerations are reported in the literature reviewed here.

Our review is based on information from a variety of jurisdictions. It is important to note the extent to which our results can be skewed by the processes by which jurisdictions come to be studied. Our experience as criminal justice researchers is that many jurisdictions are invited to participate in research but only a few agree and some of them change their mind during the implementation of the research. Better managed systems with at least adequate information systems are often attractive prospects and willing participants. On the other hand, some jurisdictions may have published reports generated about them because the authors found the lack of prosecution or conviction worthy of public attention. In either case, we think that the prospect that the jurisdictions included in this review are atypical cannot be discounted. Our samples are identified reports from jurisdictions that have been studied, not a representative sample of all jurisdictions.

Prior efforts at synthesizing the findings from scientific research have demonstrated the value of considering the potential impact of unpublished research on the review's findings (Rosenthal, 1979; Rosenthal, 1990). This file drawer problem may be diminished given the large number of studies included in this review, the even larger number of reports on intimate partner violence considered but not included, the use of multiple, automated search mechanisms, and the identification of a large number of unpublished documents. Under these conditions, the likelihood of our missing a large number of relevant reports, while possible, is limited.

Moreover, four out of our five prosecution and conviction rates are based on 40 or more reports and have mean rates of 32 percent or higher. It would take an equal number of reports showing zero prosecutions or convictions to change our conclusion about great variability in prosecution and conviction rates or substantiate the finding that prosecution or conviction for intimate partner violence is rare. However, one of our rates–convictions per reported offense–is based on only 21 reports and its mean rate (16.4%) is more susceptible to reduction if a similar number of studies showing zero convictions were found in one or more file drawers. However, unpublished reports are presumed to remain in file drawers because they do not show statistically significant effects or do not conform to the prevailing scientific consensus; given the widespread consensus about low prosecution rates, some proportion of the reports remaining in file drawers are likely to show higher, not lower, rates of prosecution and conviction.

A third major caveat to our findings is captured in the statement, *on average*. What we found is substantial variability in the rates of prosecution and conviction from jurisdiction to jurisdiction and, in some instances, within jurisdictions over time. This variability, not some essential and consistent characteristic of the criminal justice system, appears to be the basis for finding substantial amounts of prosecution and conviction for intimate partner violence. Any references to the average rates reported in this study that do not include the extent of the variability in these rates will be incomplete and inaccurate.

Future Research

Future research on the criminal justice response to intimate partner violence will be stronger if it improves upon the measurement of case processing and disposition, captures salient characteristics of studied jurisdictions, and tracks large representative samples of reported offenses to the completion of criminal justice processing over time in a large number of jurisdictions. It

seems important that future research capture information about the prosecution process from a variety of sources-- offenders, victims, advocates, prosecutors, etc. Until more reliable, empirically-based estimates of the amount of prosecution and conviction and the individual and organization characteristics that explain those amounts are available, the intimate partner violence research and reform communities might work together toward establishing the theoretical and policy rationales for the levels of criminal sanctions for intimate partner violence that are necessary for an appropriate and effective response from the criminal justice system. When better empirically based explanations about the prosecution of intimate partner violence become available, it will be essential to have more fully developed theoretical and policy contexts in which to interpret those findings.

Given that prosecution and conviction rates for intimate partner violence are higher than commonly believed, the clear imperative for social research is to expand prior efforts (e.g. Wooldredge, 2002; Jolin, et al, 1998) to establish the conditions under which prosecution, conviction and sentencing do or do not reduce future violence between intimate partners. Moreover, studies limited to crime control effects are insufficient to test theories or evaluate policies; future research needs to establish the extent to which the social benefits of these criminal justice sanctions do or do not exceed their social costs. That research agenda need not wait for more definitive estimates of the amount of prosecution or more comprehensive explanations of existing variation in the rates at which intimate partner violence results in a prosecution or a conviction.

Chapter Two

The Effectiveness of Criminal Sanctions for Intimate Partner Violence:

The First 32 Studies

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Chapter Two

The Effectiveness of Criminal Sanctions for Intimate Partner Violence: The First 32 Studies

Chapter One of this report reports a systematic review of the published literature on the amount of prosecution and conviction for intimate partner violence. This review determined that the rate of prosecution and conviction for this offense was not, as was commonly reported, "rare", infrequent," or "typically low." We found that, on average, one third of all reported offenses and three fifth of all arrests result in the filing of criminal charges. In addition, about a third of all arrests and half of all prosecutions for intimate partner violence result in a conviction.

Given the prevalence of prosecutions and convictions for intimate partner violence and the strong encouragement for the use of the criminal courts in the Violence Against Women Act, this research focused on what is known about the extent to which prosecution and conviction for intimate partner violence are and are not associated with reduced prevalence or frequency of repeat offending.

This chapter reviews the published literature on the effectiveness of using criminal sanctions to address violence between intimate partners. Among the 135 publications used to estimate the amount of prosecution and conviction in Part 1, we identified 31 publications which report 32 quantitative analyses[1] that assess the relationship between sanctions by the criminal courts and repeat offending.

[1]One publication, Peterson, 2003i, reported the results from two separate studies.

Table 2 - 1 displays (in alphabetical order by the lead author's last name) some of the characteristics of the 32 studies which report the association of criminal sanctions for intimate partner violence with repeat offending. These studies were published over a twenty year period from 1988 to 2008 based on research from 42 primarily U.S. jurisdictions[2]. Eleven of these studies sample intimate partner violence offenses; 8 sample arrests and 13 sample prosecutions. Twenty of the reported findings were derived from multivariate analyses, including one use of an experimental design and one use of hierarchical linear modeling. Ten of the findings were derived from simple bi-variate analyses and two involved before and after comparison in a single jurisdiction.

The sample sizes in these studies vary from 6,489 in three New York City boroughs during 1998 to 74 offenses reported to the police in Abbotsford, British Columbia during 1997-98. Three publications report three unique analyses from the same 3,662 cases in Hamilton County, Ohio and a fourt publication reports another analysis by the same author using1,855 of those incidents which occurred within the city limits of Cincinnati, Ohio. Combined, these 32 studies include 30,782 incidents of intimate partner violence that resulted in a prosecution, a conviction or the imposition of criminal sanctions following a conviction.

[2]Two studies (Jaffe et al., 1993; Marlsand et al/, 2001) are from Canadian jurisdictions.

In this chapter, we review this research in detail in three ways. First, we provide a short narrative of the characteristics of each study; in each of those summaries, we specify which of the three sanction hypotheses were tested and the results of those tests. Our second approach is to tabulate the reported findings from these 32 studies. For each study and for each hypothesis, we present the number of reported statistical tests[3] that show that criminal sanctions were associated with less repeat offending, more repeat offending or no effect on repeat offending. Our last approach to reviewing this literature is to describe and critique the measures, samples and analytical approaches used in these studies. Following these three detailed reviews, we conclude this chapter with an assessment of the substantive findings reported and the extent to which the methods used in these studies provide a solid basis for evaluating policy or testing theories about the effects of sanctions.

Three Hypotheses about Crime Control Effects

Our assessment is that the research literature addresses not one but three distinct hypotheses about the effects of criminal sanctions. We label these the prosecution hypothesis, the conviction hypothesis and the sanction severity hypothesis. The first two hypotheses are relatively straightforward. The prosecution hypothesis asserts that repeat offending will be reduced for offenders against whom prosecutors have filed at least one criminal charge. We conceive of prosecution as a simple dichotomy and do not consider other potentially important gradations of prosecutorial intensity, such as the number or severity of the charges. The conviction hypothesis asserts that repeat offending will be reduced for those offenders who are formally convicted by a court, either by plea or by a trial. Conviction is also conceived of as a

[3]In this project, all references to statistical significance mean significant at the .05 level.

dichotomus variable with a mixture of cases not arrested, not charged or acquitted cases in the "no conviction" category. .

The sanction severity hypothesis states that the more severe the criminal sanction the greater the reduction in repeat offending. We distinguish tests of sanction severity from tests of prosecution and conviction, even though prosecution is a more severe sanction than no prosecution and conviction is a more severe sanction than no conviction. Our conception of the sanction severity hypothesis is limited to comparisons among different types of sanctions imposed on convicted cases. Analyses that involve one test from a single variable created by rank ordering all case dispositions from the lowest to highest severity is considered a test of the sanction severity hypothesis.

When specific types of sanctions imposed following conviction are compared with cases that are prosecuted but not convicted, we interpret these as tests of the conviction hypothesis only. When specific types of sanction imposed following conviction are compares with cases that are not prosecuted, we interpret these tests as tests of the prosecution hypothesis only. This approach emphasizes the characteristic of the reference group in determining which hypothesis is being tested and it avoids double or triple counting individual tests as measures of prosecution, conviction and sanction severity hypotheses.

This approach permits distinctions among three crucial policy choices in criminal justice processing–the decision to file charges, the decision to convict, and the type of sanction imposed on convicted offenders. If there are differential effects for prosecution, conviction and severe sanctions, these distinctions would be missed if all tests of criminal sanctions are considered together. Second, the use of these three hypotheses structures how individual case outcomes are defined and which case outcome comparisons should be included in any particular analysis.

Given the great diversity in how case outcomes are defined in different jurisdictions and, perhaps more importantly, how researchers decide which types of case outcomes will be the treatment group and which ones will be in the control group, the use of these three hypotheses provides a framework to synthesize findings across a diverse research literature.

Narrative Summaries of 32 Published Studies

With the three crime control hypotheses in mind, we review the 32 studies listed in Table 2 - 1. The section below summarizes the location and time period of each study, the nature and size of the samples involved, the types of analyses employed, the ways in which recidivism is measured, and the direction and statistical significance of the reported findings. We also identify the major strengths and weaknesses of each report and how we interpret the reported findings in terms of our three hypotheses.

1. Belknap and Sullivan (2003)

Based on interviews with 178 female victims of intimate partner violence prosecuted in three counties in Colorado and Michigan during 1999 and 2000, Belknap & Sullivan (2003: 51) report statistically significant reductions in physical abuse and in psychological abuse six months and twelve months after charges were disposed in court compared to six months prior to the original arrest. We interpret these four bi-variate tests as supportive of the prosecution hypothesis.

Strengths: This report used two types of repeat offending measured directly from victim interviews at two post arrest time periods.

Weaknesses: The analyses sampled completed cases only and used a methodologically weak pre/post bivariate comparison of prevalence measures only. The report did not correlate variability in sanctions with repeat offending. While all sample sizes were small, pre-arrest

offending were based on 178 interviews and post arrest offending were based on as few as 148 interviews. Details of statistical tests were not provided.

2. Buzawa et al. 1999

Based on court documents about charges filed and initial case disposition, as well as police criminal history records and civil court restraining order documents from 353 male defendants arraigned in Quincy, Massachusetts between June 1995 and February 1996 for domestic violence, Buzawa, et al. (1999:Table 7.7) report a statistically significant increase in the prevalence of re-arrest within 12 months of the original arrest for cases receiving more severe sanctions ranging from no prosecution to jail time. We interpret this bivariate test as providing evidence against the sanction severity hypotheses.

Buzawa, et al. (1999) also report repeat violence or violations of restraining orders for 58 (49.2%) of the 118 victims interviewed at approximately 12 months after the presenting incident but they do not analyze the relationship of these measures to case dispositions.

Strengths: With a large sample of arrests in a pro-active jurisdiction, this study used official police measures to compare the prevalence of re-arrest among four distinct sanctions: cases that 1) were not prosecuted, 2) prosecuted but not convicted, 3) convicted but no jail, and 4) convicted with a jail term.

Weaknesses: Analyses reported are limited to bivariate analyses of the prevalence of arrest which do not incorporate characteristics of the pro-active jurisdiction.

3. Davis et al. (1998)

Based on 1,133 misdemeanor arrests for domestic violence presented for consideration by

the Milwaukee prosecutor during three months in 1994 and three months in 1995, Davis, et al.'s (1998: 440) multivariate analyses found no statistically significant effects on the likelihood of re-arrest for any offense within six months of the disposition of the original case comparing cases declined for prosecution with dismissed cases, cases sentenced to probation and cases sentenced to jail. We interpret the comparison of not prosecuted with declined cases as one test of the prosecution hypothesis, with probation cases and with cases sentenced to jail as two tests of the conviction hypothesis.

Strengths: This study used large samples and multivariate techniques to analyze the prevalence of re-arrests within six months of case disposition. Cases not prosecuted were compared with dismissed cases, convicted cases given probation and convicted cases given jail..

Weaknesses: No measures of the frequency of arrests or time to first new arrest. No distinction made between re-arrests for domestic violence or other offenses. Uncertain how variable time at risk addressed.

4. & 5 Dunford (1990) and Dunford et al. (1990)

Based on official records from Omaha, Nebraska on 136 misdemeanor domestic violence offenses not present when police arrived but subsequently arrested by warrant and 116 interviews with the victims in those cases, Dunford (1990) measured the time to first new offense and the prevalence and frequency of new offenses at 6 and 12 months. He reported (p. 469) that there were no statistically significant differences in the amount of repeat offending of any of his outcome measures between cases sanctioned by the court and those that were not. He also reported that there was some evidence that court sanctions may have reduced repeat offending in a companion study of 109 arrests (Dunford, et al, 1990). We interpret these findings as

nine tests of the conviction hypothesis, eight of which show no effect and one of which supports the conviction hypothesis.

Strengths: This study provides detailed measures of repeat offending from victim interviews and official records.

Weaknesses: The reported bivariate analyses are based on small samples and presented in a narrative format with no coefficients or formal statistical tests.

6. Eckberg and Podkopcaz (2002)

Based on 1,422 misdemeanor domestic violence cases filed in the Hennepin County, Minnesota Domestic Violence Court during the first nine months of 2001, Eckberg and Podkpcaz (2002:14-21) report reduced prevalence after nine months of any pretrial charges, any post disposition charges, post disposition domestic assault charges and any post disposition convictions compared to the Minneapolis section of the Hennepin County District Court during the first nine months of 1998 (N=2,098) and to suburban courts in Hennepin County (N=1,296).

Three bivariate correlation coefficients showed the association of the Domestic Violence Court reduced repeat offending but the coefficients are small and only one is statistically significant. Since the primary difference between dispositions in the Domestic Violence Court and the previous court was an increase in the conviction rate from 27 to 45 percent, we interpret these three statistical tests as providing support for the conviction hypothesis in one case and no effect in two cases.

Strengths: This study uses multiple measures and alternative times at risk to assess repeat offending in a large sample from a specialized domestic violence court.

Weaknesses: The analyses depend upon bivariate analyses of official records of new charges and

does not directly correlate case disposition with case outcomes at the individual level. No multivariate tests were reported.

7. Fagan (1989)

Based on 170 follow-up interviews with 270 women at shelters in six jurisdictions during 1980, Fagan (1989: 385) reported that the interaction between injury severity, criminal sanctions and repeat offending was statistically significant. Victims with severe injuries had lower six month prevalence rates of new violence if legal charges had been brought; the same comparison using the Conflict Tactics Scale (CTS) was not statistically significant. If the victim had severe injuries and a conviction was obtained, there were statistically significant reductions in the CTS but not in the prevalence measure. We interpret these four findings as 1) one test supporting the prosecution hypothesis, 2) one test of the prosecution hypothesis showing no effect, 3) one test supporting the conviction hypothesis, and 4) one test of the conviction hypothesis showing no effect.

Strengths: These analyses use offense based measures from victim interviews to assess not merely the direct effects of sanctions but the interaction of sanctions and victim injury on repeat offending.

Weaknesses: This bivariate analysis of a relatively small sample from five diverse sites provides no test of statistical power.

8. Finn (2004)

Based on interviews with 170 female victims of violence by male family members who were arrested in DeKalb and Gwinnett Counties, Georgia, Finn (2004:100) reports that criminal

sanctions, measured on a scale from 1 (no charges) to 6 (jail), were not associated with either outcome measure–the prevalence of abuse or the prevalence of physical violence--at six months after case disposition. We interpret these findings as two tests of the sanction severity hypothesis that show no effect for sanctions.

Strengths: These multivariate analyses are based on victim reported offenses following case disposition and include measures of victim empowerment.

Weaknesses: This study uses a single measure of sanctions and is based on a sample of 110 cases derived from a larger sample of 170 victims.

9. Ford and Regoli (1993)

Of the 686 cases of misdemeanor battery or criminal recklessness between intimate partners brought to the attention of the Marion County Indiana from July 1, 1986 to August 19, 1987, 190 cases stemming from on scene arrests were randomly assigned to three prosecutorial treatments–diversion from charges, probation and other sentences. The 480 cases stemming from victim direct complaints to the prosecutor's office were also randomly assigned to these three policies and to a fourth treatment options–permitting victims to drop charges[4].

[4]Ford & Regoli (1992, 1993) report numerous complex bi-variate and multivariate analyses of the prevalence, frequency and time to first new offense in officially recorded and victim reported repeat violence based on the direct effects of randomly assigned policy options and the interaction of actual cases dispositions and policy options. Our summary is limited to the reported findings for actual case dispositions.

Ford & Regoli (1993: 51 & 66) use a logit analysis to compare the prevalence of victim reported violence in the six months before the experimental incident with the prevalence of victim reported violence in the six months after case disposition. For both on scene arrests (N = 106) and victim complaint cases (N= 324) they report reduced prevalence of violence in the post disposition period. We interpret these findings as two tests confirming the prosecution hypothesis.

In addition, Ford & Regoli (1993:53 & 68) report no differences in the prevalence of victim reported violence based on the actual disposition of cases among on scene arrests and among cases based on victim complaints. We interpret these findings as two tests of the conviction, both of which show no effect on repeat offending.

Strengths: Based on a large sample of prosecuted cases, these analyses measure repeat offending using victim reported offenses in a period following case disposition.

*Weaknesses***:** The reported bi-variate analyses of case dispositions rely on only one measure of repeat offending.

10. Friday et al. (2006)

Based on 790 domestic violence offenses reported as part of the NIBRS program to the Charlotte Mecklenburg Police Department during 2003, Friday, et al. (2006: 38-41) use multivariate models to assess the impact of a specialized domestic violence unit and jail time on both the prevalence and frequency of repeat offending. In both tests, jail time was not related to repeat offending for a two year period. We count this as two tests of the sentence severity hypothesis with no effect. Among 448 of these incidents where the offender was arrested. Friday, et al.'s multivariate comparison (p. 59) of 272 cases with dismissed charges and 169 cases found guilty with seven cases found not guilty generated no statistically significant effects .for either

sanction. We interpret these findings as one test showing no support for the prosecution hypothesis and one test showing no support for the conviction hypothesis.

Strengths: This analysis benefits from detailed information from a large sample of arrests and a long follow-up period in a multivariate analysis.

Weaknesses: This analysis is limited to one measure of repeat offending. The analyses of all incidents is limited to testing jail time and the analyses of arrests compares hundreds of convicted cases with seven not convicted cases.

11. Frisch et al. (2001)

Based on 849 arrests for domestic violence during 1997 in four jurisdictions in New York State, Frisch, et al. 's (2001: 98) site specific multivariate analyses consistently show no effect for conviction on officially recorded new offenses during a 12 month follow-up period. We interpret these findings as four tests showing no effect for the conviction hypothesis.

*Strengths***:** Sample of offenses from four sites measured new offenses to assess repeat offending.

Weaknesses: These analyses were limited to the prevalence of one officially recorded outcome measure with cases with complete data in four out of eight sites.

12. Gross et al. (2000)

Based on data from 177 male offenders prosecuted for misdemeanor domestic violence between March and November 1997 in Chesterfield County, Virginia. Gross, et al. (2000: 309) logistic regression analysis found no statistically significant differences in repeat arrests or re-convictions over 18 months among cases that were either dismissed or given one of four types of sentences among cases that resulted in a conviction: fines, probation, suspended sentences, or jail.

We interpret these findings as eight tests of the conviction hypothesis. All of the tests show no effect.

Strengths: This multivariate analysis used two measures of repeat offending with times at risk longer than is typical for domestic violence research.

Weaknesses: This small sample study was limited to measures of the prevalence of repeat offending from official records.

13. Hartley and Frohmann (2003)

Among 189 cases in a specialized prosecution program between December 2000 and February 2002 in Chicago that generated conviction rates of 47.6%, Hartley & Frohman (2003 :95-96) report double the rates of re-arrest, re-conviction, and violations of no-contacts orders, and 50% shorter times to first new re-arrest and a 50% increase in the number of new arrests compared to 517 cases in a comparison group of domestic violence cases during the same period. The comparison group had only a 21.9% conviction rate[5]. Although no statistical tests of these differences were provided, based on the size of the effects, we interpret these findings as five tests with evidence contradicting the conviction hypothesis.

Strengths: This program used four measures of repeat offending.

Weaknesses: These analyses are bi-variate with no statistical tests. Furthermore, comparisons of conviction rates rely on aggregate rates in non-comparable treatment and control groups.

[5]In tables on pages 95-96, Harley and Frohmann focus on comparisons between cases where victims do and do not show up in court.

14. Jaffe et al. (1993)

Based on interviews with 90 victims of "wife" assault during 1988-89, Jaffe, et al (1993: 85) found statistically significant reductions in the prevalence of four CTS based measures of repeat violence over a 12 month period after charges were filed in a London, Ontario court compared to the same measures for the 12 months prior to charges being brought. Among cases where charges were not brought, Jaffe, et al. (1993) report no reductions between 12 months before and 12 months after the 1988-1989 incident for four prevalence measures. We interpret these findings as four tests providing support for the prosecution hypothesis.

Strengths: These analyses are based on changes in four victim reported measures of violence before and after the filing of charges.

Weaknesses: The bi-variate analyses do not measure the frequency or time to first failure, nor do they use official records of repeated offenses reported to the police.

15. Jolin et al. (1998)

Based on 927 arrests for intimate partner violence in Portland Oregon between March 18th and November 27th 1996, Jolin et al.'s (1998: 97-101) multivariate analyses generate the effects of prosecution on four outcome measures based on 326 victim interviews and ten outcome measures based on official records from all 927 arrests. For nine of these measures, prosecution had no effect. For three measures prosecution increased repeat offending and for two measures prosecution reduced repeat offending.

Strengths: These multivariate analyses employ large samples with multiple measures of the prevalence and frequency of repeat offending from victim interviews and official police records.

Weaknesses: The multivariate analyses do not include demographic characteristics of victims and

offenders or other sanctions besides prosecution. The full list of variables in each model and model characteristics such as sample size or explained variance are not included in this report.

16. Kingsnorth, 2006

Based on 872 arrests for intimate partner violence whose cases were disposed of by the Sacramento County Court System between January 1 and April 30, 2000, Kingsnorth's (2000: 925-26) multivariate analyses report no statistically significant effect for either the filing of charges or a conviction upon any new arrest within 18 months of the initial arrest. A separate multivariate analysis of cases that resulted in a conviction found no effect on re-arrest for the number of days sentenced to jail.

We interpret these findings as one test each for the prosecution, conviction, and sentence severity hypotheses, all of which show no effect.

*Strengths***:** These multivariate analyses are based on a large sample of arrests with a lengthy follow-up period.

Weaknesses: These analyses rely on a single prevalence measure from official arrest statistics. In addition, the presence of interaction terms in the models weakens the interpretation of the regression coefficients for criminal sanctions.

17. Klein and Tobin, 2007

Based on 342 cases from the sample of 353 used in Buzawa, et al. (1999), Klein and Tobin (2008) compiled information on the date and disposition of any subsequent arrests or newly issues restraining orders occurring within the Quincy Court by the end of 2004. In a multivariate model comparing dismissed cases with cases sentenced to probation and cases sentenced to jail, Klein &

Tobin (2008) found statistically significant increases in the prevalence and frequency of new domestic violence arrests or restraining orders associated with sentences to probation and to jail. In a similar model comparing jail sentences with dismissed cases and probation sentences, they found no statistically significant differences in either the prevalence or frequency of new arrests/orders between cases sentenced to jail and cases sentenced to probation. We interpret these findings as two tests contradicting the conviction hypothesis and two tests showing no effect for the sentence severity hypothesis.

Strengths: With a large sample of arrests in a pro-active jurisdiction and a nine year follow-up period, this study used multivariate models to compare the prevalence and frequency of re-arrest separately for convicted and nonconvicted offenders and for offenders sentenced to probation or jail.

Weaknesses: Outcome analyses based on re-arrest, not re-offending. The lengthy case disposition time might be insensitive to the short term effects anticipated by other research. The follow-up period for re-arrest begins immediately after the initial arrest, not after case disposition.

18. Marsland (2001)

Based on telephone interviews with 74 female victims of domestic violence in Abborsford, British Columbia between April 1, 1997 and March 31, 1998, Marsland (2001: 25-26) measured repeat offending in two ways: the prevalence of new assaults within 27 months and the prevalence at which those new assaults resulted in new charges being filed. Both of these rates were reduced if charges were filed compared to cases where charges were stayed. In addition, the prevalence of new assaults did not vary among cases sentenced to probation, jail or a fine. We interpret these findings as providing two tests of the prosecution hypothesis showing a crime control effect and one test of the sentence severity hypothesis showing no effect.

Strengths: These analyses determine re-offending and re-prosecution over a 27 month period based on victim interviews.

Weaknesses: The bivariate analysis of this small sample provides no tests of statistical significance or measures of repeat offending from official records.

19. Murphy et al., 1998

Based on 235 men charged in Baltimore with domestic violence related offense between January and August 1994, Murphy, et al (1998L 274-75) report that there were no statistically significant differences in the prevalence of repeat offending over a 12 to 18 month period between cases where charges were dropped or the offender acquitted, where charges were deferred or the offender was found guilty, or where the offender was sentenced to probation. This finding held when repeat offending was determined by the filing of new charges for battery or the violation of an order of protection or when repeat offending was determined by the filing of new charges for a greater variety of violent offenses. We interpret these findings as two tests of the conviction hypothesis and two tests of the sanction severity hypothesis. All four tests show no effect.

Strengths: These analyses used two measures of repeat offending from official court records over a time at risk of up to 18 months after the initial charges.

Weaknesses: These bivariate analyses used new charges, not new arrests or new offenses, had no information from victims about repeat offending and did not include measures of the frequency or time to first new repeat offense.

20. Newmark et al., 2001

Based on a sample of felony cases adjudicated in Brooklyn, New York processed in a new

specialized domestic violence court during 1995 and two comparison samples processed in non-specialized courts during 1996 and 1997, Newmark, et al.'s (2001: 72-73) analysis of the time to first new arrest following case disposition for 304 cases found no statistically significant effect for the conviction hypothesis at 12 or 18 months after case disposition. We interpret these as two tests of the conviction hypothesis showing no effect..

Strengths: This mutlivariate sample of felony cases tracked repeat offending for 18 months after case disposition.

Weaknesses: These analyses were limited to official records of repeat arrest and did not include measures of offense prevalence or frequency.

21. Orchowsky, 1999

Based on a criminal history check of 1,910 male arrested for intimate partner violence in Alexandria, Virginia from January 1, 1993 to June 1996. Orchowsky's (1999: 51) logistic regression analysis found a statistically significant increase in the prevalence of rearrest for any domestic violence offense when offenders sentenced to incarceration were compared with all other dispositions, including not guilty or *nolle prossed,* as a single group. We interpret these findings as one test with evidence against the sanction severity hypothesis.

Strengths: This multivariate analysis used a large sample with a potentially lengthy time at risk.

*Weaknesse*s: These analyses did not include evidence about the effects of criminal sanctions from victim interviews and was limited to a test of prevalence of new arrests after the original arrests (not from case disposition). In addition, the time at risk appears to vary for different offenders.

22. Peterson (2003i)

Based on a sample of 6,489 domestic violence defendants disposed by Brooklyn, Manhattan, and Queens courts during the third quarter of 1998, Peterson, (2003i: 32) found no differences in the prevalence of re-arrest for domestic violence over an 18 month period of risk comparing dismissed cases with convicted cases with no jail and cases convicted and sentenced to jail. In these boroughs the prosecutors did not permit cases to be dropped. We interpret these findings as two tests of the conviction hypothesis, both of which show no effect.

Peterson (2003i) also analyzed the effects of case disposition for 1,435 domestic violence arrests that occurred during 1998 in the Bronx, where unlike other boroughs the District Attorney permitted cases to be dropped.. Using a sample of domestic violence arrests disposed during the same time period and the same measure of repeat offending as the three borough analyses reported above, Peterson (2003i: 47) found no differences in repeat arrests when comparing cases declined for prosecution with cases dismissed, convicted cases convicted with no jail and convicted cases sentenced to jail. We interpret these findings as three tests of the prosecution hypothesis, each of which show no effect on repeat offending.

Strengths: The multivariate analyses compared a large sample of cases using a length follow-up period following case disposition.

Weaknesses: The analyses relied solely on official records and did not use frequency or time to failure parameters for repeat offending.

23. Peterson, 2004

Based on two samples--domestic violence arrests disposed in Manhattan during the third quarter of 1998 (N=990) and during the first quarter of 2001 (N=1,249)--Peterson (2004: 58)

statistically significant reports reductions in re-arrest for domestic violence over 12 months for cases convicted and sentenced to jail compared to dismissed cases. They report no differences in rearrest rates between convicted cases not sentenced to jail and dismissed cases. We interpret these findings as two tests of the conviction hypothesis, with mixed results, and one test of the sentence severity hypothesis that shows a crime control effect.

Strengths: The multivariate analyses compared a large sample of cases using a length follow-up period after case disposition.

Weaknesses: The analyses relied solely on official records and did not use frequency or time to failure parameters for repeat offending.

24. Steinman, 1988

Based on a sample of 182 men arrested for intimate partner violence in Lincoln, Nebraska between June 1 to September 30, 1986, Steinman's (1988: 181) multivariate analyses found no difference in the prevalence of re-arrest over a 12 month period between arrested cases and those cited, charged but not convicted, diverted, sentenced to probation, fined less than $100, fined more than $100, or jailed. We interpret these findings as one test of the prosecution hypothesis, four tests of the conviction hypothesis and one test of the sentence severity hypothesis–all of which show no effect.

Strengths: These multivariate analyses use a lengthy time at risk.

Weaknesses: These analyses rely on a small sample, official records of repeat offending, and prevalence measures.

25. Steinman, 1991

Based on 63 offenses reported to the police in Lancaster County, Nebraska in the year before May 31, 1986 and 273 offense reported to the same department in the year following the May 31, 1986 adoption of new coordinated pro-prosecution policies, Steinman (1991:232) reports higher prevalence in victim reported repeat offending for prosecuted cases over a 33 month period following cases in the baseline period but no differences in the same measure comparing prosecuted and not prosecuted cases in the 14 months following the experimental period. We interpret these findings as two tests, one showing more repeat offending associated with prosecution and the other showing no effect..

Strengths: These analyses involve lengthy follow-up periods among victim interviews.

Weaknesses: The narrative reporting these bivariate analyses are based on small samples, include no reports of statistical tests, and involve only prevalence measures with variable times at risk. In addition, this research collected but did not report official records of repeat offending.

26. Tolman & Weisz, 1995

Based on 341 incidents of male on female physical abuse of an intimate partner for which there was complete information on case disposition in Dupage County, Illinois, during January through March 1992, Tolman & Weisz's (1995: 490-91) multivariate analyses show no statistically significant differences for convicted cases in the prevalence of police reports of either new offenses or new arrests over an 18 months period of risk. We interpret these findings as two tests showing no effect for the conviction hypothesis.

Strengths: The multivariate analyses use two measures of repeat offending from official sources over a lengthy time at risk.

Weaknesses: Their analyses of case dispositions do not employ information from victim interviews

and do not report measures of the frequency or time to first new arrest

27. Ventura and Davis, 2005

Based on 519 prosecutions for domestic violence in Toledo, Ohio from April 2000 through March 31, 2001, Ventura and Davis's (2005: 270) multivariate analyses find statistically significant reductions in the prevalence of rearrests over a 12 month period from case disposition for prosecutions that result in a conviction compared to those prosecutions which are dismissed. We interpret these findings as one test in support of the conviction hypothesis.

In a separate multivariate analysis of sanction severity, they report that the sanction of suspended sentence or fine was associated with a statistically significant increase in the prevalence of rearrest. Since the least severe sanction was associated with increased repeat offending, we interpret this finding as one test in support for the sanction severity hypothesis.

Strengths: These multivariate analyses are based on a large sample and a lengthy time at risk.

Weaknesses: The analyses are limited to prevalence measures from official records of repeat offending.

28. Wooldredge and Thistlethwaite, 1999

Based 3,662 arrests for domestic violence in Hamilton County, Ohio during the mid-1990s, Wooldredge and Thistlethwaite (1999:71-74) compared the six categories of prosecuted arrests–those with dismissed charges, those acquitted, those in a treatment program, those sentenced to probation, those sentenced to jail, and those sentence to both probation and jail--with arrests that were not prosecuted. They used three outcome measures–the time to first new arrest, the prevalence of any new arrest and the frequency of new arrests. The analyses of prevalence and

frequency of new arrests were limited to the 3,110 arrests for which Wooldredge and Thistlethwaite had at least 24 months at risk after the case was disposed or the sentence completed. In multivariate models,15 of the 18 tests of sanction effects showed no effect. Three of these statistical tests–all comparisons of the treatment program with cases not prosecuted showed less repeat offending for the treatment cases.

We interpret these findings as 15 tests of the prosecution hypothesis showing no effect and 3 showing a deterrent effect.

Strengths: These multivariate analyses are based on a large sample, with multiple types of sanctions, use three outcome measures, and begin the measurement of repeat offending after the sanction has been completed. In addition, the multivariate models include controls for individual level stakes in conformity.

Weaknesses: This study was based on police reports of any new arrest, not offending, against any victim, not just the original victim.

29. Wooldredge, 2002

In an analysis limited to1,855 domestic violence arrests from within the city limits of Cincinnati and the prevalence of new arrests within 24 months, Wooldredge and Thistlethwaite (2002: 698) included two dichotomous variables--one for charges filed or not and one for whether defendant was convicted or not--in a two multivariate models–one that included aggregate level social characteristics at the census tract level (N= 126) and one that included those same characteristics but at the neighborhood level (N=48). These models consistently show increased re-arrest rates for cases not prosecuted but no effect for whether or not the case results in a conviction. These findings are difficult to interpret because the reference group is the common constant for all variables in each statistical model and because of the non-independence of the two

variables, since the charged cases include cases that resulted in a conviction. For our purposes, we interpret these findings as two tests supporting the prosecution hypothesis and two showing no effect for the conviction hypothesis.

Strengths: These multivariate and multi-level analyses are based on a large sample, with multiple types of sanctions, and begin the measurement of repeat offending after the sanction has been completed.

Weaknesses: This study was based on police reports of any new arrest, not offending, against any victim, not just the original victim and used only one outcome measure.

30. Wooldredge & Thistlethwaite 2002

Using the same sample of 3,110 domestic violence arrest used in Wooldredge and Thistlethwaite (1999) and one outcome measure--the prevalence of new arrests within 24 months--Wooldredge and Thisthlethwaite (2002) compare four sanctioned groups. They find that arrestees who were not prosecuted had a statistically significant higher rate of repeat offending than arrestees whose charges were dismissed or who were acquitted. They also found no difference in the rearrest rates of not prosecuted arrestees with 1) offenders sentenced to just a treatment program and 2) those sentence to a treatment program and probation or jail.

We interpret these findings as three tests of the prosecution hypothesis with two findings of no effect and one finding supporting the prosecution hypothesis.

Strengths: These multivariate analyses are based on a large sample, with multiple types of sanctions, and begin the measurement of repeat offending after the sanction has been completed.

Weaknesses: This study was based on police reports of any new arrest, not offending, against any victim, not just the original victim and used only one outcome measure with a sample that dropped

almost half of the probation cases because they did not have 24 months at risk after their term of probation was over.

31 Wooldredge and Thistlethwaite, 2005

Based 3,110 domestic violence arrests in Hamilton County, Ohio during the mid-1990s which had at least 24 months at risk after the case was disposed or the sentence completed, Wooldredge and Thistlethwaite (2005:86) used multivariate models to compare cases that were prosecuted but had charges dismissed with six other types of case dispositions–cases not charged, cases acquitted at trail, cases in an intervention program, cases sentenced to probation, cases sentenced to jail, and cases sentenced to both probation and jail. They used three outcome measures–the time to first new arrest, the prevalence of any new arrest and the frequency of new arrests.

The three outcome measures and the six comparisons with dismissed cases created 18 possible tests. For all three outcome measures, the comparison between the dismissed cases and cases not charged showed statistically significant increased rates for the cases that were not prosecuted. We interpret these findings as three tests supporting the prosecution hypothesis.

The three comparisons between cases dismissed and cases sentenced only to probation showed statistically significant reductions in all three outcome measures for the probation cases. We interpret this as three tests supporting the conviction hypothesis. However, the three comparisons between cases dismissed and cases sentenced to both jail and probation show statistically significant increases for jailed and probated cases than for dismissed cases for two out of three measures. We interpret these findings as two tests against the conviction hypothesis and one showing no effect. To complicate the analyses even more, the three comparisons of dismissed

cases with those sentenced only to jail showed no effects. We interpret these findings a three tests showing no effects for conviction.

The comparison of dismissed cases with cases jailed (but not probated) showed no statistically significant effects. We interpret this as three tests of the conviction hypothesis showing no effect. The three comparisons between dismissed cases and cases convicted and sent to a treatment program show one statistically significant reduction for the treatment cases and no effects for the other two comparisons. We interpret these findings as one test supporting the conviction hypothesis and two test showing no effect for convictions.

Lastly, because dismissed cases and acquitted cases are both prosecuted but not convicted, we make no interpretation of the three reported findings comparing these types of cases.

Summary of Substantive Findings

Our narrative review of each publication is designed to establish the diversity of samples, methods, measures used to generate the findings of the 32 studies reviewed here. In Table 2 - 2, we record the number of reported tests in each of 32 studies and whether those tests show that prosecution, conviction or sanction severity are associated with less repeat offending, more repeat offending, or no difference in repeat offending. On the basis of Table 2 - 2, in this section, we summarize the extent to which the published findings do and do not support our three hypotheses. In a subsequent section, we use the narrative reviews to establish the extent to which the samples, methods and measures used in these studies vary and address the implications of that variation on the validity and reliability of those substantive findings.

The most frequent finding in Table 2 - 2 is that criminal justice sanctions for intimate partner violence have no effect, one way or the other, on subsequent offending. Sixty-five percent (94 out of 144) of the reported tests show no statistically significant differences in the

amount of repeat offending between different types of criminal sanctions. This relationship hold for all three hypotheses: 55 percent of the prosecution tests, 76 percent of the conviction tests, and 73 percent of the sentence severity tests show no effects on repeat offending. These simple counts of significance tests favor the conclusion that criminal sanctions for intimate partner violence have no effect on repeat offending.

Although the predominant finding reported in Table 2 - 2 favors the null hypothesis, thirty five tests (24%) support one of the three crime control hypotheses and fifteen tests (10%) show contrary results--that prosecution, conviction, and severe sentences are associated with more not less repeat offending. This simple count of the published statistically significant test results provides some limited support that the idea that criminal justice sanctions can reduce violence against women. This comparison is more striking for the prosecution hypothesis for which 24 tests are supportive and only four tests show contrary results. The results for the conviction and the sentence severity hypotheses are more evenly balanced with nine supporting and nine contrary findings for the conviction hypothesis and two supporting tests and two contrary tests for the sentence severity hypothesis. The prosecution hypothesis finds support in ten studies; the contrary results come from only three studies. Similarly, the conviction hypothesis finds some support in six studies and contrary evidence comes from only two studies.

In summary, based on our detailed review of each of these 32 studies and the summary of their reported findings displayed in Table 2 - 2, we conclude that predominant finding reported in the research literature is that there is no crime control effect for the use of criminal sanctions against intimate partner violence. Support for these null findings and the limited support for the prosecution hypothesis, however, are dependent upon the relative strengths and weaknesses of the research samples, measures, and methods used in this research.

Measures, Methods and Samples Used to Test Crime Control Hypotheses

Our examination of the measures, samples and methods used in the 32 studies of the effects of prosecution on repeat offending has identified six methodological issues that appear to be sufficiently problematic that we recommend caution in using the published findings currently available to evaluate public policy or to test criminological theory. The seven methodological issues we have identified are 1) the measurement of repeat offending, 2) the measurement of criminal sanctions, 3) the analytical models tested, 4) the failure to report of statistical power, 5) the lack of attention to missing data, 6) the inattention to the potential for sample selection bias, and 7) the lack of attention to broader theories of human behavior. Individually and in combination, these methodological issues raise serious concerns about whether the reported findings should be believed, regardless of whether they show more repeat offending, less repeat offending or no effects for criminal sanctions.

Measurement of Repeat Offending

We reviewed these 31 publications for any reported measures of new criminal behavior. Our review identified how these measures of recidivism vary along six dimensions: 1) the source of the data (police records, prosecution records, or victim interviews), 2) the type of offenses included (any offense, any violence or any domestic violence, 3) the victim –offender relationship (intimate partners, other domestic relationships, any), 4) the point at which the time at risk for re-offending begins (following arrest, following case disposition, or following the completion of a sentence), 5) the time at which the time at risk ends (6 months, 12, months, 18 months, etc.), and 6) whether the analyses addressed the prevalence of repeat offending, the frequency of repeat offending or the time to first new offense.

Table 2 - 3 lists each measure for each publication and displays how each of these studies varies on these six dimensions. Forty six of these measures of recidivism are used in only one study, twelve measures are used in two studies, and five measures are used in three studies. Some publications use only one measure of repeat offending but most use two or more. Jolin, et al. (1999) report effects for 14 distinct measures.

These 32 studies in Table 2 - 3 report 85 recidivism measures. Because 19 of these measures stem from victim interviews and 20 measures from offense reports. Thirty-nine (45.9%) out of 85 measures define repeat violence as any new offense. Thirty eight (44.7%) of the 85 studies use arrest reports and define recidivism based on new arrests. The remaining nine measures used police calls for service or prosecutor or court records to measure recidivism.

In 31 measures, any type of new offense counts as a repeat while 30 measures only capture violence and another 22 are limited to domestic violence only. Two measures counted psychological abuse. In 52 measures, the victim in the recidivism event does not need to be the same as the original offense; in 33 measures, only events involving the same victim count as recidivism.

In research on the effectiveness of arrest (Sherman, 1992; Garner and Maxwell, 2000), the period of risk consistently begins on the first day after the original incident. That approach to defining recidivism is also used in 51 of the 85 measures in the literature on the effectiveness of criminal sanctions. Twenty two measures do not begin to measure repeat offending until the criminal sanctions have been decided; four measures begin at arraignment and eight measures begin at the time the offender is convicted. In one study with four publications and five measures of recidivism, the beginning of the time at risk varies for each case depending on when the case disposition or sentence (including jail time) were completed.

In this literature, when the time at risk ends is even more variable. Thirty three measures use the six month period common in the arrest studies but 15 measures use a 12 month time at risk and 12 measures use an 18 month time at risk. In one study (Klein and Tobin, 2008) with four outcome measures, the time at risk is 108 months. All measures are truncated at some point by the end of data collection and this can create variable times at risk in both official records as well as victim interviews. Seventy-three out of the 85 measures define recidivism as the prevalence of an event over a fixed period of time; ten measure the frequency of new criminal events. One measure is a time to first new event and one measure is the severity of the new event.

Figure 2 - 1 displays the measures in rank order from the lowest rate of 3.1 percent (Gross, et al., 2002) to the highest rate of 71.3 percent (Klein and Tobin, 2008). The substantial variability in how these measures of repeat offending are defined and the wide range in the actual rates of repeat offending are reported diminishes our ability to argue that the reported findings summarized in Table 2 - 3 are derived from the same underlying behavior.

Measuring Case Dispositions

The research literature reviewed here is often unclear or inconsistent about what constitutes a "prosecution" a "conviction" or what is the nature of the sentence imposed. Some studies count the filing of charges as a prosecution (Wooldredge & Thistlethwaite, 2002), but other studies report the effects of cases that are "fully" prosecuted (Kingsnorth, *et al.*, 2001). In the later understanding, cases that are dropped, dismissed or "nolle prosed" are not counted as prosecutions. Similarly disparate definitions are used in describing how a case is disposed. Among the more problematic categories include "sentencing" offenders to diversion programs or batterer treatment programs, sometimes with and sometimes without a formal conviction.

This issue is no more clearly seen than in the four publications that stem from the same data

from Hamilton County, Ohio.

Analytical Methods

Each of these studies link criminal sanctions with repeat offending but the methodological rigor in the way they make those connections varies as displayed in Table 2 - 1. Nineteen of these 32 studies tested the association between criminal sanctions and repeat offending using multivariate statistical techniques. Some of those multivariate analyses include few control variables while others incorporate more extensive considerations and even address contextual as well as individual level effects. Two publications compare repeat offending rates before and after the introduction of specialized courts. In ten studies, the reported analyses were no better than simple bi-variate comparisons and some of those analyses did not report tests of statistical significance.

In combination with the variety of outcome measures and approaches to measuring case dispositions, the diversity in analytical methods used in this research literature does not provide a solid basis for the construction of standardized effect sizes with which to assess the average effect of sanctions for intimate partner violence.

Model Development and Testing

Not only do these studies vary in how they define prosecution, conviction and sentencing severity but the statistical findings about each of these hypotheses are derived from a diverse set of statistical comparisons. Four of the seven studies that address the prosecution hypothesis (Fagan, *et al.*, 1984; Tolman & Weisz, 1995; Davis, *et al.*, 1998; Wooldredge & Thistlethwaite, 2002) conducted analyses comparing some cases where charges were filed with some cases where charges were not filed. Ford and Regoli (1993) compare alternative prosecutorial policies. Newmark, *et al.* (2001) and Hartley and Frohman (2003) report comparisons between cases processed in a specialized domestic violence court–where the prosecution of cases is more vigorously

pursued--with domestic violence cases in other courts in the same jurisdiction. There is similar diversity in how conviction cases are defined and whether convictions are compared to all arrests or just to those arrests that result in prosecution. Some of this diversity is due to the fact that some studies sample arrests and other studies only sample cases where at least some charges have been filed.

Statistical Power

None of the 32 studies in Table 2 -.3 reports the power (Cohen, 1988) of any of their statistical tests. As listed in Table 2 - 1., ten of the 32 studies have sample sizes less than 300; the median sample size is 519. Moreover, most of the analyses reported involve less than the total number of cases, either because of missing data or because of the particular comparisons reported only involve a fraction of the total cases. The lack of reporting about statistical power is a serious omission, especially since the predominant finding in this literature is "no effect" When statistical power is low (or in this case not reported at all), a finding of "no effect" can just as easily mean that the research design was not rigorous enough to detect an effect as that no effect exists. Thus, interpreting the findings reported in Table 2 - 2 is even less straightforward, given that we cannot easily interpret the 94 tests that show no effect.

Sample Selection Bias

Although the issue of sample selection bias was raised initially by Rauma (1984) and discussed extensively in the general literature on prosecution (Jacoby, 1975) and sentencing (Crutchfield, *et al.*, 1994; Zatz & Hagan, 1985), none of the published analyses of repeat offending following prosecution for intimate partner violence address this issue or acknowledge the problem as a possible caveat to their findings. The essence of this complex issue is that the process that determines whether an arrest leads to prosecution determines not only the size but the extent to

which the prosecution sample has a higher or lower risk for repeat offending. For instance, if only a small proportion of the low risk offenders are prosecuted, then the prosecuted cases can be expected to be more likely to commit a new offense than the cases that are not prosecuted, just because of the selection process. Sample selection effects can occur because of the normal operation of the criminal justice system or by the sampling methods of researchers: either way, it is important for analyses to try to separate selection effects from the treatment effects of prosecution, conviction or sentencing severity. Given the assumption that criminal justice officials are more likely to impose more severe sanction on offenders they consider more likely to re-offend, the reported findings are likely to be a combination of the effects of selection and the effects of criminal sanctions. What is reported in the studies summarized above is an unknown combination of selection and treatment effects and this is another impediment to understanding the reported findings from this body of research.

Missing Cases and Missing Data

There is also a consistent problem in this research literature with how missing data, particularly those related to missing victim interviews, are addressed. Most of these studies report that they drop cases completely from their analyses where data from one or more variable in their model are missing. While case-wise deletion is one possible method for addressing missing data, several other methods may be more appropriate and do not result in the loss of so many cases (Allison, 2001). Similarly, our review of the studies employing victim interviews as the source of outcome data found no consideration of the victim interview selection process and no attempts to address the nonrandom selection of victims who were interviewed (Brame, 2000; Maxwell, 1998).

The issues of selection bias and missing data are related. Dropping cases because of

missing data can create selection bias in the sample of cases for which results are reported. In at least some analyses, the definition of the outcome measure creates problems with missing data and selection bias. For instance, Wooldredge and Thistletwaite (1999) drop more than 500 cases from certain analyses. These cases are dropped for a number of reasons. First, one of the outcome measures Wooldredge and Thistlethwaite chose to use required 24 months at risk. Second, they chose to begin the time at risk when the case disposition was complete, including any jail or probation time. Third, they stopped collecting data on repeat offending before the 24 month period was complete. While each of these decisions can be justified and in some cases applauded, in their own right, the effect was to drop 500 cases from the analyses. Most of these involved cases sentenced to probation and, in fact, constituted half of the probation cases. Thus, because outcome data was missing, Wooldredge and Thistletwaite's methods selected 500 cases serious enough to be convicted and sentenced to probation to be dropped from the reported analyses.

Lack of Theory

The last methodological issue which impedes our ability to use the existing body of research to make reliable judgments about the effectiveness of criminal sanctions for intimate partner violence is the inattentiveness to the theoretical basis. The existing research on intimate partner violence prosecution tends to be policy-oriented and atheoretical (Ford & Breall, 2000). This practical emphasis stems the prominent role of the modern domestic violence reform movement and the strong support for prosecution and other criminal justice responses in intimate partner violence incorporated into the 1994 Violence Against Women Act. The policy orientation can be seen in Murphy, *et al.*'s (1998) focus on the effects of coordinated community responses. Newmark, *et al.* (2001) and Hartley and Frohman (2003) concentrate on differences between general and specialized domestic violence courts. Ford and Regoli (1993) compare a policy that

allows victims to file charges independent of the police and a policy that denies victims the opportunity to drop charges.

Some studies have incorporated theoretical concerns. Ford and Regoli (1993) identify victim empowerment as the underlying mechanism which explains their results. In addition, the Minneapolis Domestic Violence Experiment (Sherman & Berk, 1984), the spouse assault replication studies (Sherman, *et al.*, 1992; Garner & Maxwell, 2000), and several intimate partner violence prosecution studies (Davis, *et al.*, 1998; Thistlethwaite, *et al.*, 1998; Wooldredge & Thistlethwaite, 2002) use specific deterrence theory to articulate why the sanctions associated with criminal justice interventions might reduce repeat victimization.

In the literature on sanctions for domestic violence, the conceptualization of specific deterrence theory is under developed. Specific deterrence theory states that fear of future sanctions is the causal mechanism by which sanction affect future criminal behavior (Zimring and Hawkins, 1970). Williams (2005) and Gibbs (1975) have suggested that causal mechanisms other than fear of future sanctions might affect future criminal behavior. For instance, the imposition of a sanction may communicate that violence against partners violates social norms and an offenderIn addition, modern understanding of the etiology of violence between intimates argues that at least part of the effectiveness of criminal sanctions stems from the impact of sanctions on victim behavior. According to this line of reasoning, in jurisdictions where intimate partner violence is prosecuted and offenders convicted, victims and especially female victims, may be empowered by the opportunity to mobilize or not mobilize the authority of the criminal law.

Among these plausible alternative explanations for the effects of sanctions, the unique aspect of specific deterrence theory is that it posits changes in the level of fear within the offender as the crucial mechanism leading to reductions in repeat offending. However, none of the research

on the effects of criminal sanctions measures the offender's fear of future sanctions and that consideration is not incorporated into any of the analyses reviewed here. Some of the studies reviewed here (e.g., Ford and Regoli, 1992; Jolin, et la. 2001) incorporate the idea of victim empowerment into their interpretation of their results but they do not measure empowerment directly. In order to test these more specific theoretical mechanism, study designs need to include measures of offender fear or victim empowerment. Of course, both of these mechanisms–fear and empowerment--may be operational at the same time with the same victim-offender relationship; they may enhance each other or they may conflict, even cancel each other out.

Our approach in this review is to not attribute the reported effects to a particular mechanism, such as specific deterrence or victim empowerment, since the available research literature does not provide sufficient information to distinguish the effects of one mechanism or another. Our approach is to label association between criminal sanctions and repeat offending as a generic "crime control" effect. This approach identifies the extent to which sanctions are associated with reductions in future offending, regardless of the underlying mechanisms involved. Using this approach, our review retains its relevance to understanding the effectiveness of the current policy of promoting criminal sanctions; moreover, it does not present itself as a test specific deterrence, victim empowerment or any other micro theories when the published reports do not provide the kind of information needed for such a test. Future research would be enhanced if it were designed to do what this review cannot--generate more rigorous tests of the relative strength of these alternative micro theoretical mechanisms.

Summary of Methodological Issues

Individually and in combination these methodological problems seriously weaken the validity and the reliability of the published findings about the crime control effects of prosecuting

intimate partner violence. If these issues had been addressed, the existing studies may still have produced a diverse set of findings, maybe even the same diverse set of findings but because they were not addressed, the value of the existing research for testing theories and for evaluating policies is considerably less than it could be.

This assessment is based on a detailed review of each study, an explicit comparison of the reported findings about the effects of criminal sanctions, and an assessment of the extent to which the current research meets the basic expectations of contemporary standards for criminological research. Not only are there methodological limitations to each individual study, there are also limitations to the body of research. Our review of this body of research chronicled the lack of consistency on two crucial issues: how criminal sanctions are measured and how repeat offending is measured. If one or two studies diverted slightly from a common measure, it might be reasonable to employ some form of a standardized effect size, but that is not the case here. Given that the prevalence of re-offending derived from these diverse measures varies from 3.1% to 71.3% and the unique conceptualizations and measurements of criminal sanctions used by these studies, it is assert that this research, or any substantial subset of this research, provides a solid basis for accumulating knowledge, evaluating policy or testing theories.

The inattention to issues of statistical power in this research severely undermines the validity of the predominant findings of no effect, since many of the analytical designs may not have been rigorous enough to find an effect, if there is one. The inattention to issues of selection bias undermines not only the findings that show more severe sanctions associated with more repeat offending but all other findings as well. The potential for selection bias exists in the allocation of all types of sanctions and the reported effects for all sanction comparisons are a combination of both selection and treatment effects. Interpreting the reported findings as solely treatment effects,

without at least some controls for selection effects, invites error.

Chapter 2: Conclusions

Our detailed and critical assessment of the published research on the crime control effects of criminal sanctions for intimate partner violence leads us to conclude that, while the preponderance of the reported findings show no effect for criminal sanctions, the diversity of reported findings and the quality of the research methods and measures used in this research provide insufficient knowledge to support any conclusion about whether the widespread use of criminal prosecution and conviction for intimate partner violence does, in fact, lead to less repeat offending, more repeat offending or to no effects at all.

This assessment is more than a researcher's traditional lament for more and better research. Few among these 32 studies approximate the kind of research characteristics that were recommended over 30 years ago by the National Academy of Sciences (Sechcrest, et al 1977; Blumstein, et al, 1981); neither do they approach the methodological rigor used in the 1980s and 1990s to test the crime control effects of arrest for intimate partner violence (Sherman and Berk, 1984; Sherman, 1992; Maxwell, et al. 2001). This negative assessment is about a body of research, not just one or two studies. Each of these individual studies has strengths and some of them have considerable strengths. Our assessment is that, as a body of research, it is difficult to uses these studies to support or oppose a particular policy or to test a particular hypothesis.

If our assessment about the negative impact generated by the lack of common methods and or measures is correct, the situation is not going to be resolved by generating additional studies each with its own unique methods and measures. Perhaps with the production of hundreds of new studies, some of which with similar methods and measures to some of the existing studies, it might

be possible to begin to draw stronger conclusions about the crime control effects of criminal sanctions. Such an approach would take a lot of resources and a lot of time and would only have a chance of generating reliable findings. Perhaps the research field could agree on the appropriate methods and measures and that all future studies would have sufficient resources to successfully implement a common design. Such an approach would be a radical departure from the current structure of both the funding and the production of criminological research in general and intimate partner violence in particular. In addition, there is the problem of arriving at such agreements and then maintaining the agreed to methods and measures throughout the life of the research.

We take a different approach as described and implemented in Chapter 3. Our assessment is that the most appropriate initial approach to addressing the uncertainties of the existing published literature is to use secondary data analysis to assess the extent to which the diversity of current findings stem from how the data from the existing studies were analyzed. The question is whether it is possible to use existing data to create more common measures and more common analyses that will generate more reliable knowledge about the effectiveness of criminal sanctions.. In addition, the use secondary data analysis might explicate more clearly the nature of the published analyses, clarity the methodological issues identified in this chapter's literature review and identify new issues which should be identified in future research on the crime control effects of criminal sanctions.

Chapter Three

The Effectiveness of Criminal Sanctions for Intimate Partner Violence:

Secondary Analyses of Available Data

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Chapter Three

The Effectiveness of Criminal Sanctions for Intimate Partner Violence: Secondary Analyses of Available Data

Introduction: The Design of this Research

This chapter reports the extent to which secondary data analysis can improve our understanding of the effectiveness of criminal sanctions for intimate partner violence. A review of the published research in Chapter 1 established that a third of all reported offenses for intimate partner violence and 60 percent of all arrests for intimate partner violence result in a the filing of criminal charges. That review also established that half of all prosecutions result in a criminal conviction. In Chapter 2, our review of the 32 published studies on the relationship between criminal sanctions and repeat offending established that the predomination finding in this body of research is one of no effect. However, that conclusion is based on a body of research lacking common methods and measures and whose analytical rigor and statistical power are frequently not sufficient to detect crime control effects where and if they do exist.

This chapter is organized in five sections. First, we describe the strengths and weaknesses of using secondary data analysis to extract additional information from prior research. In the second section, we report on the availability of data from the 32 studies reviewed in Chapter Two. Using the data that are available from 12 studies, Section 3 reports on our ability to construct common measures of criminal sanctions, repeat offending, and case characteristics from the available data files.

In Section Four, we use the available data from these 12 studies to analyze the relationship of criminal sanctions to repeat offending based on common analytical methods and measures.

We summarize our findings from each site and from all 12 sites. In Section Five, we assess what we have learned about the effectiveness of criminal sanctions for intimate partner violence and the value of secondary data analysis.

Section 1: Secondary Data Analysis

Secondary data analysis (Hyman, 1972; Bryant and Wortman, 1978; Cordray and Boruch, 1981; Cordray and Orwin, 1983) is a method that uses some or all of the raw quantitative data from one or more prior studies to reproduce and perhaps build upon the originally reported analyses. The core of this project is the reproduction of the published descriptions and analyses of Wooldredge and Thistlethwaite using the archived data. The use of secondary data analysis for the reproduction of published findings by independent researchers is a contemporary standard for research quality recognized by the National Academy of Sciences (Feinberg, *et al.* 1985). Moreover, in contemporary social research, the cost of data collection far exceeds the cost of analyzing data or disseminating research findings; secondary data analysis can be an efficient and timely mechanism to assess the quality of prior research and to generate new tests of new hypotheses.

Secondary data analysis is commonplace in the field of criminology. For instance, of the 20 articles in the November 2006 through May 2007 issues of Criminology, 18 involved quantitative data analysis. Only three of those articles involved new data collection. The other 11 data analysis articles were secondary analyses of previously collected and previously analyzed data.

Secondary data analysis has limitations. First, secondary analyses are limited by the sampling and measurement used in the original analysis. While it is possible to merged similar

data from different samples (Hickman, et al. 2009; Maxwell, et al, 2002), secondary analysis is restricted to the scope and nature of the original sample or samples used. Similarly, secondary analysis is limited to the nature of the data collected and, unless new data can be linked to each case, concepts not included in the original study cannot be used in secondary analyses. However, published findings rarely exhaust all the data collected or all the appropriate ways the collected data can be used and this permits the possibility of innovative analyses within the limitations of the originally collected data.

The second major limitation of secondary data analysis is that not all researchers publicly archive the raw data from their published studies. Open access to scientific data ,is widely encouraged by scientific journals and federal funding agencies (cites), especially when those data have or might be used to inform public policy, However, conformity to those expectations is not uniform and may be honored more in the breach than in the fulfillment of those expectations.

Even when data are publicly available, the information made available may not be complete or fully documented. Crucial variables can be missing or so poorly documented that it is difficult, if not impossible, for independent analysts to use the original data to verify the original findings or to conduct additional analyses.

The primary strength of conducting secondary data analyses is that, with the difficult task of collecting the data completed, the new efforts can emphasize analytical issues. Moreover, a variety of analytical perspectives can be brought to the data analyses and, someone ironically, these additional uses helps to justify the often expensive investment in the original data collection in the first case. In addition, secondary data analyses have the potential to identify methodological and theoretical issues which were not appreciated or addressed in prior research.

Given the existence of more than two dozen published studies on the crime control effectiveness of criminal sanctions for intimate partner violence and the disparate uncertain findings from that body of research, the use of secondary data analysis offers an opportunity to critically review the nature of each of those studies in detail and to present new analyses which enhance the similarity in research measures and methods across this body of research and to do so a modest cost.

This study was designed to test the extent to which the use secondary analysis of existing data can generate a more rigorous assessment of the effects of sanctions on intimate partner violence. We anticipated improving the analyses in four ways: First, we would use more common and, if available, more appropriate measures of sanctions, repeat offending and control characteristics. Second, our design was, to the extent possible, use common multivariate methods to assess the relationship between sanctions and repeat offending. Third, our goal was to conduct tests of statistical power to determine whether the predominant finding of no effect in the published research stems the weakness of the research designs used to test sanction, not the weakness of sanctions themselves. Lastly, we anticipate that the secondary data analyses of these data will permit a new test of the extent to which at least some of the published findings stem from the selection of cases for analysis and not merely from the effects (or lack of effects) from criminal sanctions. At the present time, each of these four issues limits our confidence in the published finding. The design of this research is to either eliminate these issues or establish the extent to which the diversity of published findings stem from the diversity of measurement and analyses, weak statistical power, or the lack of controls selection bias.

Section 2: Obtain Existing Data

Secondary data analysis is not possible without the availability of the raw data from the original studies. Encouragement for the sharing of raw data comes from many sources. Many scientific journals, especially those in economic, require authors to make their data available for re-analyses. Funding agencies, such as the National Science Foundation and the National Institute of Justice include data archiving requirements in the research awards. In addition, research methodologists have identified the reproducibility of prior research as an essential element defining science from other disciplines.

Despite these encouragements, we were able to obtain at least data for only 14 of the 32 studies that have published findings on the effectiveness of criminal sanctions on intimate partner violence. Data for 11 of these studies were available from the Criminal Justice Archive at the University of Michigan. Data for another three studies (Davis, et al., 1998; Fagan, 1989; and Ford and Regoli, 1993) were obtained for use in this project and we appreciate their willingness to assist us in this effort. These data are currently available from the NACJD. Our ability to utilize the data files from these 14 studies benefitted from the assistance of the NACJD. Several crucial variables in some of the archived data files are only available to researchers who demonstrate a need and a capability to use potentially confidential information in a protected environment and we were able to make use of these restricted data for this research.

Tables 3 - 1A and 3 - 1B lists the authors sample size, and jurisdiction for those studies for which data were and were not available. The relationship between studies reported in Chapter 2 and data collections listed in Table 3 - 1A is complicated. First, the four studies by Wooldredge and Thistlethwaite are based on one set of data. Second, since Wilson and Kleinn used a ten year

follow-up of the cases reported by Buzawa, et al. (1999), we used the updated data only. The two Dunford samples were collected as part of a common study and are treated here as one data collection. The data obtained from Davis only included repeat offending data for cases that were prosecuted, so the sample size of available data is only 643. In addition, we added two new data collections. Hirschel, et al. (1991) and Weisz, et al. (2001) collected and archived data on repeat offending and criminal sanctions for intimate partner violence but they have not published findings about the effects of prosecution, conviction or sentence severity on repeat offending. The 12 data collections used in this research generated 11,518 cases. The available data represents thirty seven percent of the 31,136 potential cases from the prior research on intimate partner violence.

The authors of the other 18 studies that had not archived their data were contacted by phone, by email and, in some instances, in person but they are unable or unwilling to share their data with us. The unavailability of data from those 18 studies demonstrates one of the weaknesses of secondary data analysis. Just as literature reviews and meta-analysis are limited to published findings, secondary data analyses are limited to studies with available data. This failure is all the more unexpected given that only five of the studies without available data were published before 2000 and many of them were supported by awards from the National Institute of Justice whose grant conditions require public archiving of research data.

This research and this research approach would have been stronger if we had been able to obtain access to all the data used in the 31 published studies about the effectiveness of criminal sanctions. The unwillingness of authors, journals and funding agencies to ensure the availability of research data unfortunately adds to limitations of this research literature and creates another

reason why the substantive findings summarized in Chapter 2 do not provide a solid basis for evaluating policies or testing theories. Information in over half the studies and from two thirds of the cases cannot be verified and therefore have limited scientific value.

Section 3: **Create Common Measures**

As a result of our efforts, we obtained access to data files from 12 independent data collections. Each of these data collections and the studies that have been generated from them are reviewed in detail in Chapter 2 and will not be reviewed again here. As displayed in Figure 3 -1, these 12 collection vary not only in where they were conducted but also in how large a sample was obtained. At 170 cases, Newton's 2001 less than five percent of the size of Wooldredge's 3.662 cases in Cincinnati. Six of the 12 data collections have fewer than 500 cases. While total sample size is only one indication of the strength of a research study, is an important component of any test of statistical power. Moreover, there are methodological complications of using studies with such disparate sample sizes to synthesize research findings.

As displayed in Table 3 - 1A, these 12 data collections sample different types of incidents. Six sample offenses; four sample prosecutions, and two sample arrests. One observation from conducting our secondary analyses is the importance of the nature of the sample and which types of incidents are and are not included in the comparison with incidents that are prosecuted, convicted or jailed. For instance, studies with offense samples can compare incidents where the offender is not arrested or not prosecuted with incidents where the offender is sent to jail. Analyses using prosecution samples cannot conduct such comparisons. This issue is addressed in our subsequent discussion of hypotheses and common analyses.

We have used data from each of those collections to create common measures of criminal sanctions and common measures of case characteristics. We identified 11 distinct behaviors of law enforcement officers, prosecutors, and judicial institutions that were recorded in some or all of these data files. These 11 behaviors are used to describe the nature of the criminal justice system response to intimate partner violence, we focus our analytical efforts on three distinct sanctions, being prosecuted, being convicted, and being jailed.

We also identified 10 characteristics of the offender, the victim and the criminal offense that were recorded with some consistency in these data files that had some policy and theoretical relevance to the nature and extent of repeat offending. These characteristics were developed based on our understanding of prior research and the theoretical bases for identifying predictors of repeat offending. The selection here, however, was driven as much by what was available as by what measures we would have preferred to have.

In this study, the available data provide for more consistency in the measurement of criminal sanctions than for the measurement of offender, victim and case characteristics. Our review in Chapter 2 demonstrated the extent to which the published studies use disparate measures of sanctions, case characteristics and repeat offending. One of the potential strengths of secondary data analysis is that it permits the opportunity to use existing data from multiple studies in a more consistent manner than the original analyses conducted by independent researchers. The creation of common measures through secondary data analyses is not likely to be a replacement for creating common measures as part of the design of original studies, but it may permit improvements over the less structured products on independent research. However, these

strengths are only potential until we demonstrate the extent to which we can use the existing data to create such common measures.

Criminal Sanctions

Despite the great diversity of sanction measures used in the published literature, we were able to use the archived data to group types criminal sanctions into commonly used legal and policy categories of prosecution, conviction, and jail sentence. In Tables 3 - 2A and 3 - 2B, we display for each of the 12 data collections, the number and percentage of cases for which a warrant was issues, an arrest was made, charges were filed, charges were deferred, charges dismissed, an acquittal was issued, the offender was convicted of at least one charge, and whether the convicted offender was sentenced to a batterer program, a fine, probation or to jail. These tables describe the percentage of each sanction type occurred in each study. The distribution of sentences reported in Tables 3 - 2 A & B are also displayed for each site in Figure 3 - 2 using just four categories–Arrested, Charged, Convicted, and Jailed. In general, in 10 of these studies, the charged cases are a subset of those arrested. This is not the case in the Ford & Regoli and the Fagan studies, where prosecutors allowed charges to be brought without arrests being made. In Ford and Regolia, only 25% of the prosecuted cases stemmed from an arrest. These differences are not particularly salient in a study of prosecution and conviction but warrant additional attention in studies which either assume all prosecuted cases result in an arrest or which sample only arrests and ignore prosecutions without arrests.

One factor contributing to the complexity of the criminal justice system is that one offender could have many if not all of these events occur as part of the consideration of one or more criminal charges about a single incident. In Table 3 - 3, we have listed the number and

percent of incidents for which each type of sanction was the most severe sanction imposed.. We ranked sanctions in the following order: no arrest, arrest only, charges filed but dismissed, charges filed but resulted in an acquittal, a conviction with an unknown sentence, convicted with a financial penalty only conviction with a probation sentence, or conviction with at least some jail time imposed. This ranking conforms to a fairly common understanding of the relative severity of case dispositions and it permits us to consistently code known disposition from each of the 12 data collections.

We use this ranking to generate across studies a consistent sanction severity measure. In Table 3 - 3, we have also grouped each of the disposition categories under two broad headings–charged not/not charged and convicted/not convicted. This categorization is intended to provide a smaller number of consistently used disposition types and to provide a common and consistent basis for testing the prosecution hypothesis and the conviction hypothesis.
Using common definitions, Table 3 - 3 displays the differences in the most severe sanction imposed in these 12 studies. This range of cases for which the most severe disposition was not even an arrest varied from 66 percent in Fagan to 0 percent in six of the 12 studies. At the other extreme of severity, 57 percent of the cases in Brooklyn studied by Newmark and her colleagues received a jail sentence; in five of these 12 studies, no more than 4 percent received a jail sentence. In Figures 3 - 3A, B and C, we display for each study, the percentage of cases for which each of the 9 categories used in Table 3 - 3 are the most severe sanction imposed. While the primary purpose of this study is not to explain variations in sanction rates, we think it is necessary to appreciate the variability of sanctions imposed between studies when trying to assess

the extent to which site specific variability in sanctions explains variation in the rates of repeat offending within that site.

There are some limitations to our ranking of case dispositions according to severity. In our analyses, we required that a disposition to a treatment program or even to probation include a formal conviction. In some jurisdictions, prosecutors file charges and agree to drop them if the offender meets certain condition such as attending a treatment program or paying financial restitution. Many of our studies included dispositions of deferred prosecution; however, none of these studies indicated whether their coding meant that the original disposition was a deferred prosecution or that failures under an original deferred prosecution were re-coded based on the final disposition of the cases. Of course, the difficulty may not stem less from the lack of clarify of coding documentation and more from the lack of documented follow-up by the criminal justice system following deferred prosecution. For the purpose of ranking dispositions in this research, we assumed deferred prosecutions were the same disposition as dismissed charges. Unless explicitly described as a conviction, we considered disposition of a financial payments or a treatment program to also have been a dismissal.

In some instances, the archived data would have information about case dispositions in multiple variables. For instance, a study might have a general disposition variable as well as a variable for time sentenced to probation or time sentenced to jail. In general, these variables were consistent but when there were conflicts, we coded the disposition with the most severe disposition category. For instance, it the sentence variable indicated a conviction with a fine but the jail time variable indicated 30 days in jail, we coded the case as a jail sanction.

In general, our conceptual approach emphasizes working across studies to generate consistent disposition categories and an explicitly hierarchy of categories that can be collapsed into simple dichotomies of prosecution and no prosecution, conviction and no conviction, jail and no jail. This approach ignores some complexity in actual case dispositions. It makes no distinctions between one day in jail and five years in prison or one month on probation or five years on probation. These can be real differences to offenders and to victims. In the analyses presented here, we have ignored this potentially important variation in sanctioning policies. Future research that is able to incorporate these differences might provide improved understanding of the effectiveness of sanctions, but they were beyond the scope of this effort.

Our use of the available data and the construction of consistent measures of criminal sanctions led us to expand our thinking about structuring our research around the prosecution, conviction and severity hypotheses. In Chapter 2, the value of this conceptual distinction was justified on the reasonable prospect that there was not just one consistent crime control effect regardless of the type and severity of the criminal sanction. Our review of the prior research established that there were some differences in the amount of support for prosecution, conviction and sentence severity hypotheses.

Our understanding of crime control hypotheses has changed in two ways. First, we noticed the variety of sample types in the published literature. Some sampled offenses, some sampled arrests, some sampled prosecutions and some sampled convictions. If the research based on an offense sample examined the prosecution hypothesis, the not prosecuted group included offenders who were and were not arrested. If the research was based on an arrest sample, the not prosecuted group was limited to arrested offenders. Within in the same study, it

is possible to conduct two tests of the prosecution hypothesis, one with and one without the "not arrested" group of offenders. This observation led to another consideration. Each of our hypotheses is defined as much by how the comparison group is defined as it is by how the treatment group is defined. A comparison between prosecuted cases and arrested cases may not be the same test as a comparison between prosecuted cases and offenders who were not arrested.

This thinking has led us to construct a three tiered hierarchy of hypotheses. Under the general hypothesis that crime is controlled by sanctions, we originally had specified three more specific hypotheses–prosecution, conviction and sanction severity. Our current thinking is that under these three sets of hypotheses are several more detailed hypotheses. These more detailed hypotheses are defined by the group of cases with which the sanctioned group is compared.

The full hierarchy is set out in Table 3 - 4 where we label these more detailed hypotheses by both the sanction and the comparison group. For instance, the first detailed hypothesis under the prosecution hypothesis is labeled, prosecution given offense, which is distinguished from prosecution given arrest. In this formulation, the dichotomous measure of jail and no jail as well as the categorical measure ranking all sanctions are both conceived of as measures of sentence severity. At this point, the benefits of these more detailed distinctions are speculative, as are the distinctions between prosecution, conviction and sentence severity sanctions. However, the use of secondary data analysis provides the opportunity to test the broader hypotheses and the more detailed hypotheses using data from the same study.

Outcome Measures

Our review of the published literature in Chapter 2 established the variety of ways in which researchers have conceived repeat offending and the total lack of consistency among the

published analyses in how repeat offending was measured.. An advantage of secondary data analysis is that it is possible to not only use the measures used in the published research but to also use the available data to generate a more consistent set of measures that can be used in two or more studies. In addition to reproducing the original findings and generating consistent analyses across studies, it is possible to construct hundreds of alternative measures of repeat offending from these 12 data collections. However, our design is to construct measures from each of the 12 available data collections. Because most of these studies report the prevalence of repeat offending from official police records, we limit our efforts here to those measures. Some of these measures are the same as those used in the original publications; some are similar measures but with different types of victims, different criteria for failure, different types of offenses, and different times at risk for repeat offending.

Table 3 - 5 displays the outcome measures used in the published studies from the 12 data collection and the outcome measures we use in our analyses. For instance, Davis, et al. 1998 use a single outcome measure derived from police records whose time at risk ended six months after the case was disposed. They included new arrests of the same offender for any type of offense and against any victim for a six month period. This is a perfectly reasonable approach and we use the available data to reproduce that measure of repeat offending. We used the available data to create another measure, exactly the same of the one used by Davis, et al., except that it measures police reports of new offenses and it not limited to situations where the police made an arrest.

We think that arrest behavior is produced by police policies and practices and may not be as good a measure of repeat offending as reported offenses. Davis, et al., do not report analyses off the offense measure, nor do they report that they collected data on offenses. It is difficult to

compare either our arrest based or our offense based measure with the published study because it does not report a base rate for offending and because repeat offending data is not available for all 1,133 cases.[1]

We report a similar comparison with the outcome measures used by Dunford, 1990 and Dunford, et al., 1990. These two publications use police records to measure the extent for any offenses or arrests that were recorded by the police against the same victim from the date of the original offense for six and 12 months. Again we were able to construct those same measures but also measures that limited the definition of repeat offending to violent offenses. Differences in the use of offenses and arrests, any victim or the same victim, any offense or violent offenses can be crucial considerations in assessing the impact of criminal sanctions for intimate partner violence.

Frequently, researchers using official records are limited to the capacity of the local criminal history record systems to distinguish consistently between types of victims, types of offense, and dates of events. But where information about offenses are available, they are to be preferred over arrests and it is preferable to distinguish between offenses against the same victim and offenses against other victims. In most of the prior research on police initiatives, the period at risk for repeat offending began the day after the initial contact. Among the published research on the effects of post-arrest criminal sanctions, some studies (Davis, et al., 1998, Ford & Regoli, 1992, Newmark, et al., 2001, and Wooldredge & Thistlethwaite, 1999, 2002, 2005) do not begin the time at risk for repeat offending until after the case is disposed. In Cincinnati, Wooldredge &

[1]The available data was split into two files, one for dismissed cases and one for prosecuted cases. The file for the dismissed cases did not include the data used to create their outcome measure. For this reason, they used 1,133 cases and we used 643 cases.

Thistlethwaite wait until the completion sentences, such as probation and jail, to being the time at risk.

We think there is a strong logic for initiating time at risk at the time of case disposition or sentence completion, where that data is available. The logic is strong–we should not expect criminal sanctions to have crime control effects until they are known, imposed and completed. Where we found that the necessary data was available, we constructed our analyses using the date of disposition as the beginning of the time at risk. To construct these analyses, we needed to know when cases were disposed and when sentences were complete. As displayed in Table 3 - 5, we were able to construct outcome measures beginning at disposition in the Charlotte data generated by Hirschel, et al. (1990), the Orchowsky, (1999) data , and the Wilson & Klein(2006) data.

There are complications when reconstructing the time at risk in available data, especially when using prevalence measures with a fixed time at risk. Unless the criminal records are searched or researched later than is typically done, starting the time at risk when the disposition is decided or the sentence is imposed and completed can easily result in the need to drop cases from certain analyses. This can happen with originally collected data (Wooldredge & Thistlethwaite, 1999) and result in dropping of almost 50% of the cases sentenced to probation from some analyses. There is another potential limitation to beginning the time at risk after sentence completing. Often an offender's criminal behavior while waiting for a case to be decided will affect how the case is decided. Offenders sentenced to jail may be offenders who committed additional offense after the initial arrest. The approach we have taken here ignores failures between the initial offense or arrest and final case disposition.

Most of the published findings in these studies use a six month period of time at risk. A few use 12 or 18 months. The data collected by Wilson & Klein, 2006, have up to 108 months at risk. As displayed in Table 3 - 5, they report a prevalence measure for 108 months[2] but do not report analyses based off the traditional six or 12 months. For purposes of comparison, we construct measure based that begin at the time of case disposition and end at six, 12, and 108 months.

Case Characteristics

The published findings about the prevalence of repeat offending from from 12 data collections used here are all multivariate analyses. While data collection was included here because they had measures of case disposition and repeat offending, we made no requirement that they included any particular set of case characteristics for use in our multivariate analyses. We addressed this issue but identifying 15 case characteristics which had been used in many of these analyses and appeared to be present in most of the 12 data collections. We compiled information on the age, race and sex of the offender and the victim, whether the couple were intimate partners, married, had children together or lived together. We used the available data to determine if the offender was using alcohol at the time of the offense, was employed or had a previous arrest. Lastly we determined if the victim was injured or whether felony charges had been filed.

Table 3 - 6 displays for each the 12 data collection the relevant data for each of these 15 case characteristics. This same information is presented in graphical form in Figures 3 - 4A, B, C and D. In general, there was fairly complete information for most case characteristics for most studies, with some major exceptions. Except for Davis, et al. 1998, there was virtually no

[2]Klein & Tobin, 2008 do report various analyses based on the time to first repeat offense.

missing data for victim and offender sex and for race of offender. Where known, the age, race and sex of offender and victims were highly correlated.

Offender employment was totally missing in two studies (Weisz, et al 2001, Davis, et al., 1998) and missing in half the cases in Jolin, et al. 2001. Five studies were missing information on whether there were children in the family in 50% or more of the cases. Virtually all of the cases were and were known to involve intimate partners. In ten sites, more than half the cases were married but this information was missing in all of the Fagan, 1999 cases and half of the cases in Jolin. Information about whether the offender and victim were living together was missing completely in three studies but available for nearly all the cases in nine studies. Eight of the 12 data collections had no information on suspect use of alcohol and five had missing data about felony charges on more than 50% of the cases. Information about victim injuries was completely unavailable in two studies, missing in 35% of the cases in one study but available in virtually all the other cases in the other nine sites. Only the Orchowsky, 1999 study did not have information on prior arrests and only Davis, et al., 1998 did not have either the age of the victim or the age of the offender. Data on the employment of the offender was missing in more than 50% of the cases in four sites; in the eight sites with some employment data, most offenders were employed.

While the age of offenders and victims consistently averaged about 30 years across sites and most cases involved male offenders and female victims, the case characteristics varied substantially on the prior arrest record, whether the charges were felony or misdemeanors, and whether the victim was injured.

Section Four: Create Common Analyses

We constructed common measures of sanctions, outcome measures and case characteristics in order to demonstrate the extent to which secondary data analyses can improve our knowledge about the effects that criminal sanctions based on published research. For the 12 data collection for which data were available, we were able to construct common measures of criminal sanctions and to construct a variety of measures of repeat offending.

We created common measures for case characteristics but were far less successful in finding the information we needed in the available data for every case or for every site. For some variables, more data were missing than present. This problem was faced by the original studies and many of them chose to report their multivariate findings with a limited number of variables or by using a subset of the study's total cases. In the 32 studies reviewed in Chapter 2, missing data were addressed in two ways. Either the variable was not included in the analyses or cases were excluded from the analyses.

We chose another approach to incorporate case characteristics into our multivariate analyses in a more consistent manner. First, we used only one race, one sex, and one age variable. We included the remaining 12 case characteristics into all our analyses. Where data were missing completely, the variables were deleted from the multivariate analysis. Where data were partially missing, we coded each variable as 1 for yes and 0 for no and counted all missing data as no. Where the age of the offender was missing, we substituted the mean age for cases with data. This approach did not require us to delete cases with missing data.

While there are more sophisticated approaches to handling missing data, our goal was not to obtain or report the relationship of case characteristics on repeat offending. Our design was to

use secondary data analysis to test the extent to which and the conditions under which sanctions affect repeat offending. Our goal is to determine whether the inclusion of case characteristics changes the direction or statistical significance of the sanction effects generated by simpler models with just sanction variables.

Our analytical design has two components. First, we seek to use the available data to conduct tests of the prosecution, conviction and sanction severity hypotheses for each site. Based on those results, we will summarize the results of all these tests by counting all the tests supporting each hypothesis, finding no effect, and finding evidence that contradicts the hypothesis. We first conduct a two variable logistic regression model using only the sanction variable and the measure of repeat offending. We then add the available data on case characteristics to whether the sanction variable effects are enhanced or diminished.

We assess the extent to which the direction and the statistical significance of the reported findings vary depending upon the use of alternative samples, alternative measures of criminal sanctions and repeat offending as well as the inclusion of case characteristics. This approach provides an easy comparison to the findings generated by reviewing the published literature but it suffers many of the same weaknesses we identified in Chapter Two. In addition, this approach assumes that all reported tests or tests that are possible with available data are of equal value.

Our second approach is to specify what we see as the relative strengths and weaknesses of the available statistical models in each study and to propose those tests which are most appropriate for assessing the effectiveness of criminal sanctions on repeat offending among the analyses available. Our preference is to obtain information about repeat offending from victim interviews and to construct analyses of the rate of offending controlling for time at risk. Few of

the 32 published studies collected victim interviews or the number of repeat offenses. Where victim interview data are available, they tend to not be representative of the entire sample of cases. Our assessments about methodological quality in this report are about those studies with police records (or only victim interview in the case of Fagan's five site study). These studies represent the large proportion of the 32 published studies on the effectiveness of sanctions on repeat offending for intimate partner violence and this focus provides the best opportunity to use secondary data analysis to assess this body of research.

This approach involves judgment about the nature of the measures used and the relevance of the analytical designs to testing theories and evaluating policies. In this approach, we are the relative methodological strength among the possible analyses. This approach is intended as an alternative to treating all possible analyses of equal value.

We follow the format used in Chapter Two. We first present a narrative description of the methods, measures and results from each site. Based on these site specific summaries we generate a tabular summary of the findings from all 12 sites similar to that presented in Table 2 - 3.

To distinguish our efforts from those of the original authors, we present our narrative and tabular summaries here in alphabetical order of the site and refer to the results by site not by original author. The results summarized below are derived from a data file on each of the reported logistic regressions that is available from the National Archive of Criminal Justice data.

Narrative Summaries of Secondary Analyses

Alexandria, VA, 1993 - 96

Using the existing data on repeat arrests for domestic violence from a sample of prosecutions in Alexandria, Virginia, we conducted four tests of the conviction hypothesis and four tests of the prosecution hypothesis. We used the available data to construct measures of repeat offending beginning with the completion of the case disposition. This approach resulted in the reduction of the original sample size from 1910 to 1556 for the tests of conviction and to 1287 for the tests of being sent to jail. We were able to use 9 of the case characteristics as controls.

The coefficients for the conviction tests were all negative and the coefficients for the jail tests were all positive but none of these tests came close to the .05 level of statistical significance. The inclusion of case characteristics did not change either the direction or the statistical significance of the findings. We interpret these findings as four tests of the conviction given prosecution hypothesis and four tests of the jail given prosecution hypothesis, none of which showed any effect on the prevalence of re-arrest for domestic violence.

Brooklyn, NY, 1997

Using existing data on repeat arrests any offense, any violence or for contempt of court in Brooklyn's Felony domestic violence court, we conducted six tests of the conviction hypothesis and six tests of the jail hypothesis using 170 cases for which 12 months of repeat offending data were available. We expanded the number of offense types to be investigated and included tests of sentencing felony offenders to jail. Our analyses included 10 case characteristics

The findings show no effects for either sanction types on any offense type and no effect of including case characteristics into our models. We count this as 12 tests of sanctions showing no effects.

Charlotte, 1987-88

Using the available data from the Charlotte Domestic Violence Experiment, we constructed measures of repeat offending for 521 cases for six months from the disposition of the case derived from arrest records for any type of crime and for violent crime and for any victim and for the same victim. From this offense sample, we tested the prosecution, conviction, jail and sentence severity hypotheses and were able to incorporate 10 case characteristics into our multivariate models. With two offense types, two victim types and two multivariate models, these data permit us to test the prosecution hypothesis eight times, the conviction hypothesis eight times, the jail hypothesis eight times and the sentence severity hypothesis eight times.

The results were consistent. The prosecution, conviction and jail coefficients were all negative but not statistically significant. All but one of the sentence severity measure coefficients was positive and none of them were statistically significant. Thirty-two tests and no statistically significant effects found.

Charlotte, 2003

Using NIBRS offense data from the Charlotte-Mecklenberg Police Department, we constructed measures of repeat offending ending 6 months after case disposition for 880 cases. The analyses is based on new offenses of any type reported against anyone and new domestic violence offenses reported against anyone or the same victim. From these data we generated six analyses of the prosecution, conviction, jail and sentence severity hypotheses and incorporated 10

of the 12 case characteristics as controls. Twenty-four tests and no statistically significant effects found.

Cincinnati, 1995

Using the only data available to measure prevalence of re-arrest for domestic violence against any victim over a 24 month period, we conducted six tests using the arrest sample of 3,110 cases, four tests using a prosecution sample of 2,882 cases, and two tests using a conviction sample of 1,002 cases. Using different samples permitted us to test the following hypotheses: prosecution given arrest, conviction given arrest, conviction given prosecution, jail given arrest, jail given prosecution, and jail given conviction. The two prosecution coefficients are negative and statistically significant. The two conviction given arrest coefficients are also negative and statistically significant. However, the two conviction given prosecution coefficients are negative but not statistically significant. All six of the jail coefficients are positive and statistically significant.

We interpret these findings as two tests supporting prosecution hypothesis, two tests supporting the conviction hypothesis, two tests showing no effect for the conviction hypothesis, and six tests contradicting the three jail hypotheses.

Detroit, 1998

Using data from 1,050 domestic violence offense reported in Detroit during 1998, we used the only two measures of repeat offending available–arrest for domestic violence against any victim or the same victim in the six months from the original offense. With the offense sample, we produced four tests of the prosecution given offense hypothesis, four tests of the conviction given offence hypothesis, and four tests of the jail hypothesis. All of the prosecution and

conviction test coefficients are positive and not statistically significant. The jailed coefficients are also positive but three out the four are statistically significant. The test with the measure using any victim and no statistical controls had a p value of .054. When the nine statistical controls are added, the p value drops to .027.

When we use the sample of 118 prosecutions, we find that the four tests of the conviction given prosecution hypothesis are not statistically significant; one of the coefficients is negative, the other three are positive. However, the tests of the jailed given prosecution hypothesis show statistically significant effects for three out the four tests. The coefficients are all positive but the bi-variate test with any victim has a p value of .07. When the statistical controls are added, the p value drops to .04.

The two tests of the jailed given conviction hypothesis show positive coefficients for both tests but, in this instance, the bi-variate shows a statistically significant effect at p = .015 but the the p value rises to .98 when the nine statistical controls are added to the model. [Note: These analyses involve 64 convictions of which 14 offenders were sentenced to jail].

Five Sites, 1980

Using the available data from 270 interviews with shelter participants in five sites, repeat offending was measured only as a violent offense against the same victim within six months of the initial offense. We tested bi-variate and multivariate models for prosecution, conviction and jailed hypotheses in an offense sample. All six coefficients were negative and all the effects were not statistically significant. [The two p values for the conviction tests were .062 and .058 respectively]. Two tests for the sentence severity hypothesis showed a mixture of signs and no

statistically significant effects. Two tests of the conviction, jail and sentence severity hypotheses using a prosecution sample showed no statistically significant effects.

Marion County, IN, 1987

Based on the available data from 430 interviews with victims about their subsequent victimization within 6 months of the original contact with the Marion County Prosecutors office, we conducted two tests of the conviction given prosecution hypothesis and two tests of the sentence severity given prosecution hypothesis, The coefficients for the conviction and sentence severity tests were positive but not statistically significant; however, in both severity tests, the coefficient for the "convicted but not sentenced to probation" were negative and statistically significant.

Milwaukee, WI, 1994

With the data available about the 643 prosecuted domestic violence cases in Milwaukee in 1994, we constructed measures of new offenses and new arrests for any offense and for any victim for the six months following case disposition. Four tests of the conviction given prosecution hypothesis found results in mixed directions and with no statistically significant effects. Tests of the jail given prosecution hypothesis found no effects when repeat offending was measured by new arrests but when it was measured by new offenses the bi-variate positive effect had a p value of .034; when the six statistical controls were added, the p value increased to .068.

Four tests of the conviction given jail hypothesis, four tests of the severity given prosecution hypothesis and four tests of the severity given conviction hypothesis all showed mixed signs with none of them being statistically significant.

Omaha, NE, 1986

The data available from Omaha, Nebraska provides numerous alternative measures of repeat offending. It includes measures of repeat offending and repeat arrests. It includes data on any type of offense and on violent offenses. It begins the time at risk at the initial offense and the time at risk ends at both 6 and 12 months. There are 32 tests of the prosecution given offense hypothesis and another 32 tests of the prosecution given arrest hypothesis. While these 64 coefficients vary from positive to negative, none of them are statistically significant. And the results are the same for the 64 tests for conviction given offense and conviction given arrest--no statistically significant effects.

The 32 tests of the jail given offense hypothesis, on the other hand, generated only positive coefficients and 12 of them show statistically significant effects. The arrest sample had a mixture of positive and negative coefficients but the six statistically significant effects showed an escalation in offending. The 16 tests for the severity given offense hypothesis show only one test that generated statistically significant positive effects for the entire variable but in 12 of those tests, the coefficient for the comparison between jail and not being arrested was positive and statistically significant.

Portland, OR, 1996

The data available from Portland, Oregon permit the use of three measures of repeat offending–the report of any offense against any victim, any office against the same victim and a violent offense against the same victim. The time at risk for all measures is 6 months from the original arrest. With an arrest sample of 927 cases, the data permit six tests of the prosecution

given an arrest hypothesis, six tests of the conviction given arrest hypothesis, six tests of the jail given arrest hypothesis, and six tests of the severity given arrest hypothesis.

In 5 out of 6 tests, prosecution, conviction, and jail are associated with more repeat offending when the measure of repeat offending is any offense against any victim. In four out of four tests, conviction and jail are associated with more repeat offending when the measure of repeat offending is any offense against the same victim. In six out of six tests, prosecution, conviction and jail are not associated with more or less repeat offending when the measure of repeat offending is violence against the same victim. A similar pattern holds for the six tests of sentence severity. When the two more general measures are used, the more severe the sentence, the risk of repeat offending is greater and statistically significant in three out of four tests.

Quincy, MA, 1995

The available data for Quincy, Massachusetts captures repeat offending for nine years, one year and six months past the disposition of the case for any victim for either any offense or for violent offenses. For all 12 tests using the nine-year time at risk measure, prosecution, conviction and jail are associated with increased amount of repeat offending, regardless of which measure is used. For eight of the eight tests of conviction and jail using the one year measure, these sanctions are also associated with higher levels of repeat offending. For all 12 of the tests using the measures of new violence against the same victim, there are no differences in the prevalence of repeat offending following prosecution, conviction or jail time. Lastly, for one out the four tests using the measure using the one year follow-up, prosecution is associated with increased levels of repeat offending.

Summary Findings from Secondary Data Analysis

In Table 3 - 7, we summarize the findings reported for each of the 12 sites involved in this secondary data analysis. This table counts whether each of the reported tests shows statistically significant reductions in repeat offending, statistically significant increases in repeat offending or no effects at all. The results are fairly striking. Only four of the 370 tests show a reduction in repeat offending and those findings come from one study. Fifty-eight tests show criminal sanctions are associated with increased amounts of repeat offending. The predominant finding is that 308 of the tests show no effect, one way or the other.

Section 5: Conclusions

These findings were not what we expected. We had anticipated that the distribution of effects generated from our secondary data analyses would look something like the results found in Detroit and Cincinnati, with a mixture of positive and negative coefficients and tests of statistical significance that would change depending on the measure used or the nature of the statistical controls included in the multivariate analyses. We had anticipated the need to try and explain the patterns of positive, negative and statistically significant effects.

The existence of only two tests supporting the prosecution hypothesis is a dramatic shift from the large number of tests that supported the prosecution hypothesis in the prior research (See Table 2 - 3.. This shift is partially due to the four publications from the Cincinnati data, which may have inflated the evidence on the effectiveness of prosecution in Chapter Two.

We had also anticipated that there would be at least some variation in the results associated with the various prosecution, conviction and sentence severity hypotheses and that

these distinctions would help identify the source of some of the disparity in findings in the published literature.

We had anticipated that the large number of tests that did not reach statistical significance might be explained by small sample sizes and low statistical power. However, studies with large sample sizes as well as studies with small sample sizes consistently show no statistical effects. We had worried that the small numbers of cases in some of the sanctions categories might cause additional concerns about statistical power. This concern was allayed when the methodological literature on statistical power consistently asserts that, in multivariate analyses, statistical power is a function of the total sample (Cohen, 1980).

There is at least one issue that this research was not able to address. It is well known that the criminal justice system selects offenders for more severe sanctions, at least in part, based on an assumption of greater risk of future offending. At a minimum, offenders are not randomly assigned to different sanctions. Under these conditions, it is difficult to separate out the increased risk due to the criminal justice selection process and the change in behavior that stems from the nature of the treatment assigned. To address this issue, future research needs to be designed to both explain why certain offenders were given more or less severe sanctions and which offenders re-offended. Until that issue is better addressed, we cannot definitively state we the results summarized in Table 3 - 7 represent the criminogenic effects of more severe sanctions or the current practice of assigning higher risk offenders to more severe sanctions. It is likely that both effects are going on to some degree but we cannot tell which is the dominant effect among intimate partner violence.

The findings of this research provide no basis for recommending the enhanced use of criminal sanctions for intimate partner violence, if the rationale for such policies is the reduction of subsequent violence among the sanctioned offenders. The evidence from our secondary data analysis is limited to 12 sites but it in no site did our re-analysis find support for the prosecution, conviction or sentence severity hypotheses that had not been reported in the published literature. Moreover, our secondary data analysis generated more findings contradicting these hypotheses or finding no effect than had been evident in the published findings.

There are other rationales for promoting the use of criminal sanctions, such as the general threat conveyed by the imposition of sanctions on individuals. The imposition of the criminal law also communicates to members of the society that a particular type of behavior is morally wrong and not now or never has been acceptable. Criminal sanctions can reduce offending through a general threat or through the moral message conveyed in ways that the research data collected and analyzed here cannot test.

Future research would be stronger if it addressed the effects of selection processes and the treatment effects of sanctions and/or treatments in the same study. Past research has emphasized the effects of sanctions and treatments on the individuals receiving these sanction and treatment and has been inattentive to the more diffuse but not necessarily any less power general effects. One more study of the specific effects of sanctions may not be as valuable as a more innovative study examining how the general population is and is not influenced by the sanctions imposed on others.

Table 1 - 2: Incident Based Prosecution and Conviction Rates without Outliers

All Known Reports with Outliers Excluded	Prosecutions Per			Convictions Per	
	Reported Offense	Arrest	Reported Offense	Arrest	Prosecution
Average of Incidents	35.5%	57.6%	16.4%	30.5	47.8
Outliers Excluded	3	0	2	0	1
Number of Reports	40	53	22	50	99
Reported Offenses	79,146		25,215		
Arrests		94,998		84,736	
Prosecutions	28,104	54,695			144,652
Convictions			4,125	25,872	69,102

Table 1 - 3: Report Based Prosecution and Conviction Rates

All Known Reports	Prosecutions Per		Convictions Per		
	Reported Offense	Arrest	Reported Offense	Arrest	Prosecution
Mean of Reports	34.4%	59.2%	16.4%	32.0%	51.2%
Number of Reports	43	53	24	53	100
Minimum	2.7%	4.6%	4.0%	0.4%	8.1%
Maximum	79.0%	98.0%	53.1%	65.1%	98.9%
Median	27.4%	63.0%	14.5%	32.0%	50.2%

Table 1 - 4: Seventeen Reports with All Five Prosecution and Conviction Rates

Reports with All Five Rates	Prosecutions Per		Convictions Per		
	Reported Offense	Arrest	Reported Offense	Arrest	Prosecution
Mean of Reports	27.1%	54.6%	15.8%	30.9%	55.7%
Number of Reports	17	17	17	17	17
Minimum	3.3%	13.8%	0.5%	1.4%	10.2%
Maximum	62.0%	98.0%	53.1%	61.3%	89.0%
Median	22.3%	52.7%	11.0%	27.7%	52.0%

	Prosecutions Per		Convictions Per		
Reports with All Five Rates	Reported Offense	Arrest	Reported Offense	Arrest	Prosecution
Mean of Incidents	17.2%	49.6%	10.2%	29.4%	59.3%
Reported Offenses	22,030		22,030		
Arrests		7,651		7,651	
Prosecutions	3,797	3,797			3,797
Convictions			2,252	2,252	2,252

Table 1 - 5: U. S. Prosecution and Conviction Rates Over Time

Prosecution and Conviction Rates	Correlation Over Time		Before 1995		1995 & Later		Absolute	Relative
			N	Rate	N	Rate	Difference	Difference
Prosecutions Per Reported Offense	0.241	27	16	26.3%	11	29.6%	3.3%	12.7%
Prosecutions Per Arrest	0.219	41	16	53.0%	25	67.1%	14.1%	26.6%
Convictions Per Reported Offense	-0.132	19	9	15.9%	10	13.1%	-2.8%	-17.8%
Convictions Per Arrest	0.041	43	20	30.5%	23	35.2%	4.7%	15.3%
Convictions Per Prosecution	-0.001	74	23	48.9%	51	50.4%	1.5%	3.0%

Table 1 - 6: Prosecution and Conviction Rates in U.S. and Other Jurisdictions

Prosecution and Conviction Rates	U.S.		Not U.S.		Total		Absolute	Relative
	N	Rate	N	Rate	N	Rate	Difference	Difference
Prosecutions Per Reported Offense	27	27.6%	16	45.7%	43	34.4%	-18.1%	65.5%
Prosecutions Per Arrest	41	61.6%	12	50.7%	53	59.2%	10.9%	-17.7%
Convictions Per Reported Offense	19	14.4%	5	17.6%	24	16.4%	-3.1%	21.8%
Convictions Per Arrest	43	33.0%	7	25.6%	50	32.0%	7.3%	-22.2%
Convictions Per Prosecution	75	49.9%	25	53.8%	100	50.9%	-3.9%	7.7%

Table 2 - 1 : Intimate Partner Violence Prosecution Studies with Analyses of Repeat Offending

Authors	Jurisdictions	Sample Size	Sample Type	Start	Stop	Types of Analyses
Belknap & Sullivan 2002	Three U.S. Counties	178	Prosecution	1999	2000	Pre-Post
Buzawa, et al. 1999	Quincy. Mass.	353	Prosecution	1995	1996	Bivariate
Davis, et al. 1998	Milwaukee	1,133	Prosecution	1994	1995	Multivariate
Dunford 1990	Omaha, Nebraska	136	Offense	1986	1987	Bivariate
Dunford, et al. 1990	Omaha, Nebraska	109	Offense	1986	1987	Bivariate
Eckberg & Podkopacz 2002	Minneapolis & Suburbs	4,816	Prosecution	1998	2001	Pre-Post
Fagan 1989	Five Jurisdictions	170	Offense	1978	1979	Bivariate
Finn 2003	Two Georgia Counties	110	Arrest	2002	2002	Multivariate
Ford & Regoli 1993	Marion Co., Indiana	642	Prosecution	1986	1987	Bivariate
Friday, et al. 2006	Charlotte	826	Offense	2003	2003	Multivariate
Frisch, et al. 2001	Six New York Sites	849	Offense	1997	1997	Multivariate
Gross, et al. 2000	Chesterfield Co., Virgina	177	Prosecution	1997	1997	Multivariate
Hartley & Frohmann 2003	Chicago	706	Prosecution	2000	2001	Bivariate
Hirschel, 1991	Charlotte	650	Offense	1986	1987	Not Reported
Jaffe, et al. 1993	London. Ontario	90	Offense	1988	1989	Bivariate
Jolin, et al. 1998	Portland. Oregon	927	Arrest	1996	1996	Multivariate
Kingsnorth 2006	Sacramento	872	Arrest	1999	2001	Multivariate
Klein & Tubin, 2008	Quincy, Mass.	342	Prosecute	1995	1996	Multivariate
Marsland, et al. 2001	Abbotsford. B.C.	74	Offense	1997	1998	Bivariate
Murphy, et al., 1998	Baltimore	235	Offense	1994	1994	Bivariate
Newmark, et al. 2001	Brooklyn	304	Prosecution	1997	1997	Multivariate
Orchowsky 1999	Alexandria	1,910	Offense	1996	1997	Multivariate
Peterson, 2003i	Three NYC Boroughs	6,489	Prosecution	1998	1998	Multivariate
	Bronx	1,435	Prosecution	1998	1998	Multivariate
Peterson 2004	Manhattan	2,239	Prosecution	1998	2001	Multivariate
Steinman 1988	Lancaster Co., Neb.	182	Arrest	1985	1986	Multivariate
Steinman 1991	Lancaster Co., Neb.	306	Offense	1986	1986	Bivariate
Tolman & Weisz 1995	Dupage Co., Ill.	341	Prosecution	1992	1992	Multivariate
Ventura & Davis 2005	Toledo	519	Prosecution	2000	2001	Multivariate
Wooldredge & Thistlethwaite 1999	Hamilton Co., Ohio	3,662	Arrest	1993	1996	Multivariate
Wooldredge & Thistlethwaite 2002	Hamilton Co., Ohio	3,110	Arrest	1993	1996	Multivariate
Wooldredge 2002	Cincinnati	1,855	Arrest	1993	1996	Hierarchical Linear Model
Wooldredge & Thistlethwaite 2005	Hamilton Co., Ohio	3,662	Arrest	1993	1996	Multivariate

Table 2 - 2: Outcome Measures Used in Intimate Violence Prosecution Research (Page 1 of 5)

				Recidivism	Reported		
Report	Data	Time at Risk	Type of Behavior	Sample	Base	Months	6 Month
Author/Date	Source	Starts at	Repeat Incident	Size	Rate	At Risk	Rate
Belknap & Sullivan	Victim Interviews	Disposition	Any CTS Item	160	38.1%	6	38.1%
				148	34.9%	12	17.5%
			Psychological Abuse	160	m = 1.11	6	N.A.
				148	m = .99	12	N.A.
Buzawa, et al., 1999	Police Records	Arraignment	Any Arrest	353	47.9%	12	24.0%
			Arrest for violence; same victim		22.1%	12	11.1%
			Arrest for violence; not same victim		10.8%	12	5.4%
			Arrest for nonpersonal offense		15.0%	12	7.5%
	Victim Interviews		Violence or Violation of Restraining Order	118	49.2%	12	24.6%
Davis, et al., 1998	Police Records	Disposition	Any arrest	1,133	N.R.	6	N.R.
Dunford, 1990	Police Records	Incident	Arrest; Same Victim	247	8.9%	6	8.9%
					16.2%	12	8.1%
			Reports; Same Victim		18.6%	6	18.6%
					28.7%	12	14.4%
	Victim Interviews		Same Victim Pushed/Hit	196	58.2%	6	58.2%
					62.2%	12	31.1%
Dunford, et al, 1990	Police Reports	Incident	Arrest; Same Victim	330	10.6%	6	10.6%
			Reports; Same Victim		16.7%	6	16.7%
	Victim Inteviews		Same Victim Pushed/Hit	242	40.5%	6	40.5%

Table 2 - 2: Outcome Measures Used in Intimate Violence Prosecution Research (Page 2 of 5)							
Report	Data	Time at Risk	Type of Behavior	Recidivism	Base	Months	6 Month
Author/Date	Source	Starts at	Repeat Incident	Sample Size	Rate	At Risk	Rate
Eckberg & Podcopcaz, 2002	Police Records	Disposition	Arrest	6,187	30.7%	9	20.5%
			Arrest for Domestic Violence		14.1%	9	9.4%
			Conviction		14.8%	9	9.9%
			Conviction for Domestic Assault		4.6%	9	3.0%
Fagan, et al.,1984	Victim Interviews	Dispostion	Violence	149	32.2%	6	32.2%
Fagan, 1989		Incident	CTS plus sexual assault	270	28.5%	6	28.5%
Ford & Regoli, 1992b	Police Records	Disposition	Violence	642	3.1%	6	3.1%
	Victim Inteviews		Violence	430	30.9%	6	30.9%
			CTS Violence		34.2%	6	34.2%
			CTS Severe Violence		20.5%	6	20.5%
Friday, et al. (2006)	Police Records	Incident	Any Domestic Violence Offense	766	34.1%	24	8.5%
Frisch (2001)	Police Records	Incident	Any Offense	6,803	36.6%	18	12.2%
			Domestic Violence Offense		31.6%		10.5%
			Aggrevated Offense		19.6%		6.5%
			Aggrevated DV Offense		15.7%		5.2%
Gross, et al., 2000	Police Records	Incident	Any Arrest	177	24.3%	18	8.1%
			Any Conviction		19.8%	18	6.6%
Hartley & Frohmann, 2003	Police Records	Incident	Any arrest	706	28.8%	6	28.8%
			Arrest for Domestic Violence		12.0%	6	12.0%
	Victim Interviews	Disposition	Kicked, bit or hit with fist	47	10.6%	6	10.6%
Jaffe, et al, 1993	Victim Interviews	Incident	Pushed, grabbed or shoved	90	50.9%	12	25.5%
			Slapped		28.3%	12	14.1%
			Kicked, hit, or bit		26.7%	12	13.4%

Table 2 - 2: Outcome Measures Used in Intimate Violence Prosecution Research (Page 3 of 5)							
Report	Data	Time at Risk	Type of Behavior	Recidivism	Base	Months	6 Month
Author/Date	Source	Starts at	Repeat Incident	Sample Size	Rate	At Risk	Rate
Jolin, et al., 1998	Police Records	Incident	Prevalence of Re-victimization	883	14.0%	6	14.0%
			Prevalence of Arrest from Revictimization		7.8%	6	7.8%
			Prevalence of Re-offense		13.6%	6	13.6%
			Prevalence of Arrest from Re-Offense		8.0%	6	8.0%
			Frequency of Re-vcitimization		m = .20	6	m = .4
			Frequency of Arrest from Revictimization		m = .08	6	m = .16
			Frequency of Re-offending		m = .22	6	m = .44
			Frequency of Arrest for Re-offending		m = .08	6	m = .16
			Any Repeat Calls to same address		46.0%	6	46.0%
			Repeat Call for DV at same address		14.0%	6	14.0%
	Victim Interviews		Prevalence of victimization same offender	395	60.8%	6	60.8%
			Prevalence of victimization any offender		61.3%	6	61.3%
			Frequency of revictimization same offender		m = 3.28	6	m = 6.56
			Frequency of revictimization any offender		m = 3.41	6	m = 6.82
Kingsnorth, 2006	Police Records	Incident	Arrest for Intimate Partner Violence	872	15.3%	18	5.1%

Table 2 - 2: Outcome Measures Used in Intimate Violence Prosecution Research (Page 4 of 5)							
Report	Data	Time at Risk	Type of Behavior	Recidivism	Base	Months	6 Month
Author/Date	Source	Starts at	Repeat Incident	Sample Size	Rate	At Risk	Rate
Klein & Tobin, 2008	Police Records	Incident	Arrest for Any Offense	342	65.5%	108	3.6%
			Arrest for Domestic Violence		50.9%		2.8%
			Arrest for Domestic Violence or Restraining Order Issued		59.9%		3.3%
			Frequency of Arrest for Domestic Violence or New Restraining Order		m = 1.94		
Marsland, 2001	Victim Interviews	Incident	Assault	74	43.0%	27	9.6%
Murphy, et al., 1998	Police Records	Incident	Battery or Violation of Protection Order	235	15.7%		
			Violent Offense		25.5%	12 to 18	N.A.
Newmark, et al., 2001	Police Records	Disposition	Any Arrest	304	31.3%	12	15.7%
			Arrest for Violent Felony		4.9%	12	2.5%
			Arrest for Criminal Contempt		11.1%	12	5.6%
			Any Arrest		39.9%	18	13.3%
			Arrest for Violent Felony		7.2%	18	2.4%
			Arrest for Criminal Contempt		15.2%	18	5.1%
Orchowsky, 1999	Police Records	Incident	Domestic Violence Offense	1910	21.0%	variable	N.A.
Peterson, 2003	Police Records	Disposition	Any Arrest for Domestic Violence	6489	17.0%	18	5.7%
Peterson, 2004	Police Records	Disposition	Any Arrest for Domestic Violence	2134	14.2%	18	4.7%

Table 2 - 2: Outcome Measures Used in Intimate Violence Prosecution Research (Page 5 of 5)							
Report	Data	Time at Risk	Type of Behavior	Recidivism	Base	Months	6 Month
Author/Date	Source	Starts at	Repeat Incident	Sample Size	Rate	At Risk	Rate
Steinman, 1988	Prosecutor Records	Incident	Charged with Physical Violence or Threats	183	19.7%	12	9.8%
Steinman, 1991	Police Records or Victim Interview	Incident	Domestic Violence Offense	338	61.5%	Not Reported	N.A.
	Victim Interviews				59.5%		
Venture & Davis, 2005	Police Records	Disposition	Arrest for Domestic Violence	519	32.6%	12	16.3%
Wooldredge & Thistlethwaite, 1999; 2005	Police Records	Completion	Prevalence of Arrest for Domestic Violence	3110	14.4%	24	3.6%
			Frequency of Arrest for Domestic Violence	3110	m = .19	24	N.A.
			Time to First Arrest for Domestic Violence	3662	m = 10.0	Variable	N. A.
Wooldredge, 2002	Police Records	Completion	Arrest for Domestic Violence	1855	14.0%	24	3.5%
Wooldredge & Thistlethwaite, 2002	Police Records	Completion	Arrest for Domestic Violence	3110	14.0%	24	3.5%

Table 2 - 3: Number of Reported Statistical Tests About the Effectiveness of Sanctions for Intimate Partner Violence

		Prosecution			Conviction			Sentence Severity		
Study	Jurisdictions	Less	None	More	Less	None	More	Less	None	More
Belknap & Sullivan, 2003	Three Counties	4	0	0						
Buzawa, et al., 1999	Quincy							0	0	1
Davis, et al., 1998	Milwaukee	0	1	0	0	2	0			
Dunford, 1990	Omaha				0	8	0			
Dunford, et al., 1990	Omaha				1	0	0			
Eckberg & Podcopcaz, 2002	Minneapolis				1	2	0			
Fagan, 1989	Five U.S. Sites	1	1	0	1	1	0			
Finn, 2003	Dekalb & Gwinnett							0	2	0
Ford & Regoli, 1993	Marion Co.	2	0	0	0	2	0			
Friday, 2006	Charlotte				0	1	0			
Frisch, 2001	Four New York Sites				0	4	0			
Gross, et al., 2000	Chesterfield Co.				0	8	0			
Hartley & Frohmann, 2003	Chicago				0	0	5			
Jaffe, et al, 1993	London	4	0	0						
Jolin, et al., 1998	Portland	2	9	3						
Kingsnorth, 2006	Sacramento	0	1	0	0	1	0	0	1	0
Klein & Tubin, 2008	Quincy	0	0	0	0	0	2	0	2	0
Marsland, et al., 2001	Abbotsford	2	0	0				0	1	0
Murphy, et al., 1998	Baltimore				0	2	0	0	2	0
Newmark, et al., 2001	Brooklyn				0	2	0			
Orchowsky, 1999	Alexandria							0	0	1
Peterson, 2003i	3 NYC Boroughs				0	2	0			
Peterson, 2003i	Bronx	0	3	0						
Peterson, 2004	Manhattan				1	1	0	1	0	0
Steinman, 1988	Lancaster Co.	0	1	0	0	1	0	0	3	0
Steinman, 1991	Lancaster Co.	0	1	1						
Tolman & Weisz, 1995	Dupage Co.				0	2	0			
Ventura & Davis, 2005	Toledo				1	0	0	1	0	0
	Subtotal	15	17	4	5	39	7	2	11	2
Wooldredge & Thistlethwaite, 1999	Hamilton Co.	3	15	0						
Wooldredge, 2002	Cincinnati	2	0	0	0	2	0			
Wooldredge & Thistlethwaite, 2002	Hamilton Co.	1	2	0						
Wooldredge & Thistlethwaite, 2005	Hamilton Co.	3	0	0	4	9	2			

Direction Reported	Less	None	More	Less	None	More	Less	None	More
Summary of Effects	24	34	4	9	50	9	2	11	2
Number of Studies	16 out of 32			21 out of 32			10 out of 32		

Figure 2 - 1A: 85 Reported Recidivism Rates in 31 Studies
(Rates Between 3.1 Percent to 19.8 Percent)

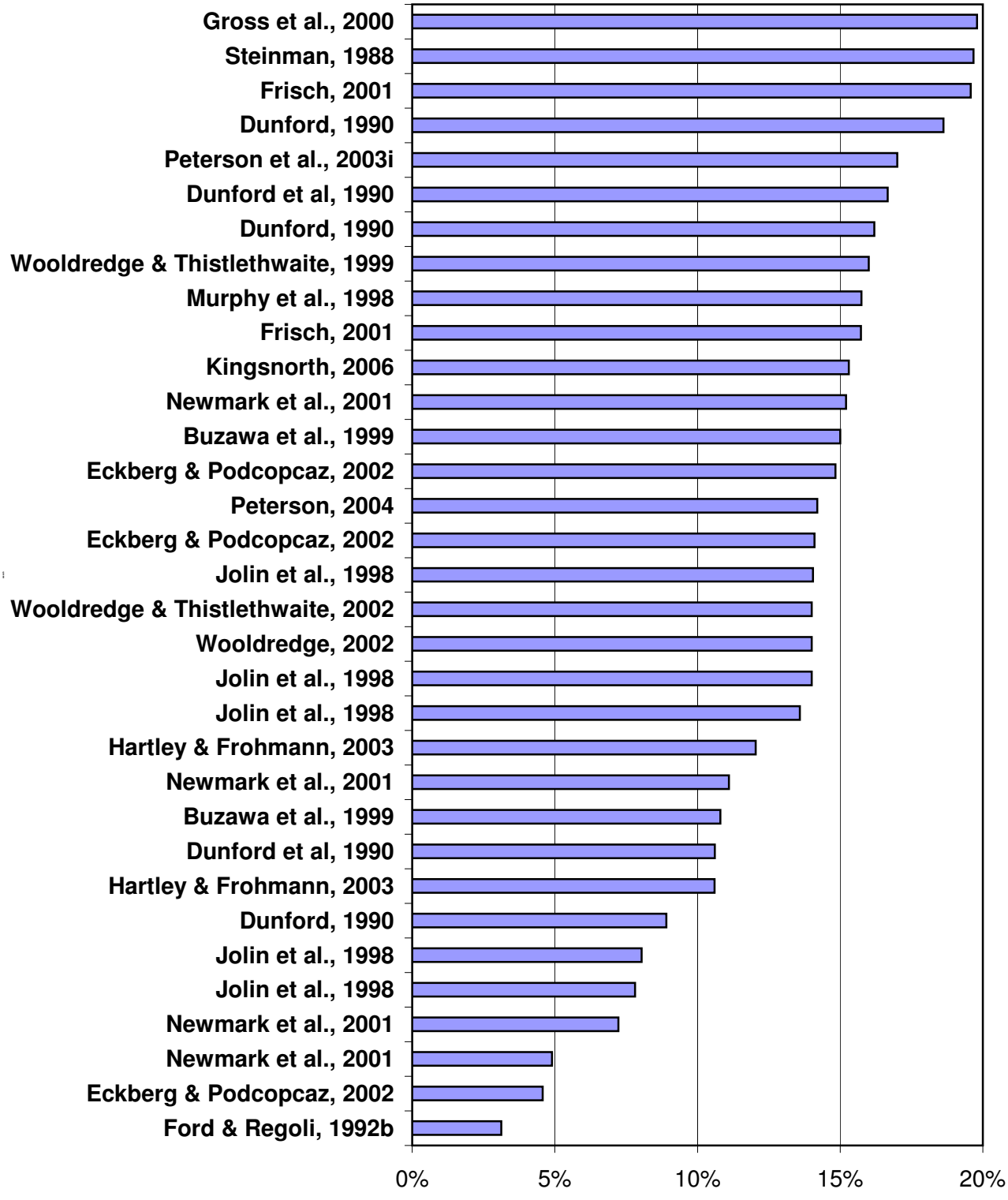


Figure 2 - 1B 85 Reported Recidivism Rates in 31 Studies
(Rates Between 20.5 Percent to 71.3 Percent)

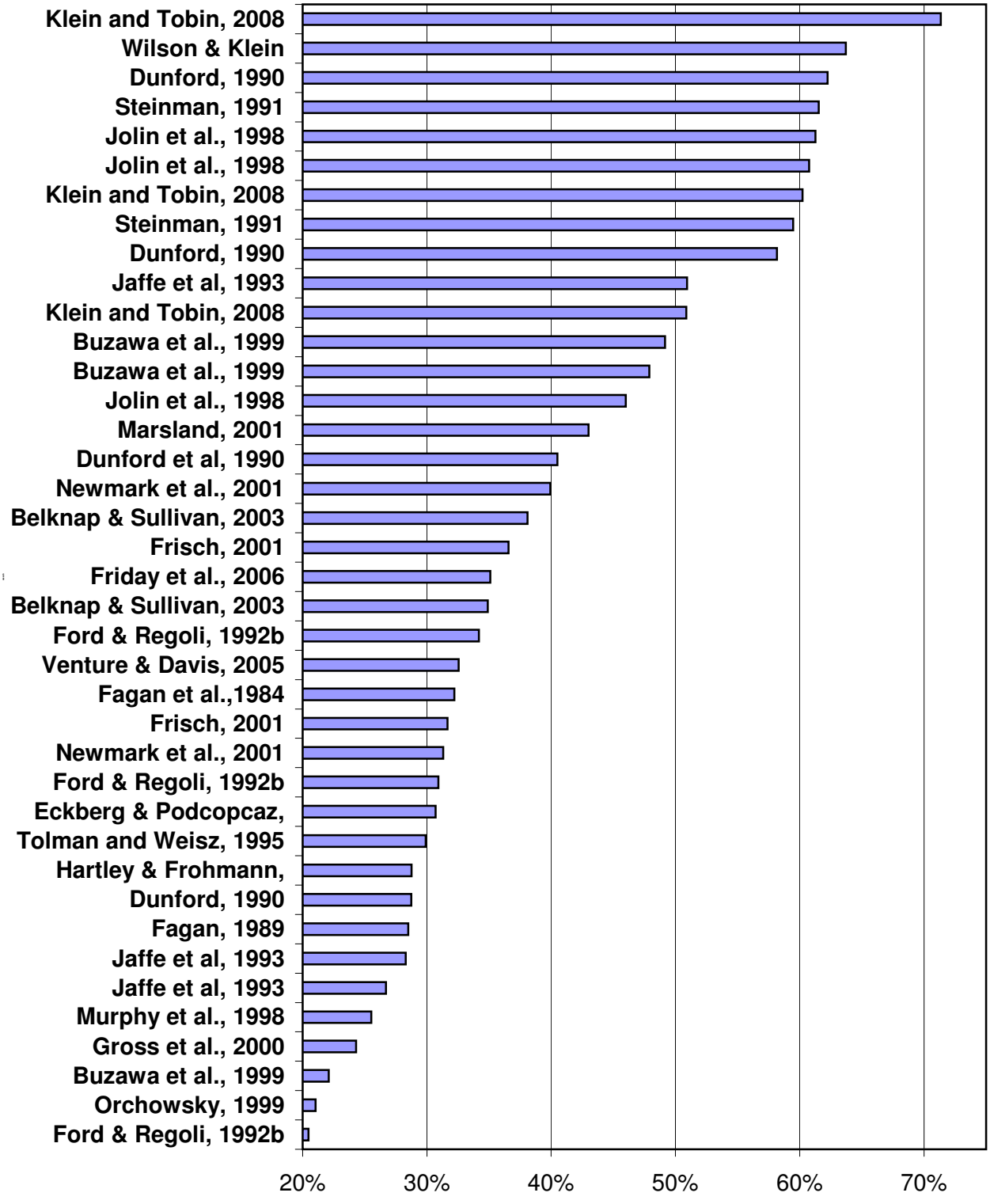


Table 3-1A: Studies with Data on Sanctions and Repeat Offending

Studies with Available Data	Sample Size	Sample Type	Jurisdictions	Data Availability
Davis, et al., 1998	643	Prosecution	Milwaukee	Partially Obtained
Dunford, 1990	247	Offense	Omaha	Archived
Dunford, et al., 1990	330	Offense	Omaha	Archived
Fagan, 1989	270	Offense	Six U.S. Sites	Obtained
Ford & Regoli, 1993	430	Prosecution	Marion Co.	Obtained
Friday, 2006	880	Offense	Charlotte	Archived
Hirschel, 1991	650	Offense	Charlotte	Archived
Jolin, et al., 1998	927	Arrest	Portland	Archived
Newmark, et al., 2001	170	Prosecution	Brooklyn	Archived
Orchowsky, 1999	1,910	Prosecution	Alexandria	Archived
Weisz, et al., 2001	1,057	Offense	Detroit	Archived
Wilson & Klein, 2006	342	Offense	Quincy	Archived
Wooldredge & Thistlethwaite, 1999	3,662	Arrest	Hamilton Co.	Archived
Total Cases	11,518	All Sites With Data		

Table 3 - 2A Sanction Rates by the Studies

Sanctions		Studies											
		Davis, et al.		Dunford		Fagan		Ford & Regoli		Friday		Hirschel	
		N	%	N	%	N	%	N	%	N	%	N	%
Total Cases		643	100.0%	577	100.0%	270	100.0%	430	100.0%	880	100.0%	650	100.0%
Arrest Warrant Issued	No	643	100.0%	469	81.3%	Unknown		106	24.7%	601	68.3%	0	0.0%
	Yes	0	0.0%	108	18.7%	Unknown		324	75.3%	279	31.7%	0	0.0%
Arrested	No	0	0.0%	391	67.8%	246	91.1%	324	75.3%	436	49.5%	223	34.3%
	Yes	643	100.0%	186	32.2%	24	8.9%	106	24.7%	444	50.5%	427	65.7%
Charges Filed	No	0	0.0%	447	77.5%	196	72.6%	0	0.0%	488	55.5%	482	74.2%
	Yes	643	100.0%	130	22.5%	74	27.4%	430	100.0%	392	44.5%	168	25.8%
Charges Deferred	No	643	100.0%	575	99.7%	Unknown		312	72.6%	0	0.0%	0	0.0%
	Yes	0	0.0%	2	0.3%	Unknown		118	27.4%	0	0.0%	0	0.0%
Charges Dismissed	No	427	66.4%	547	94.8%	Unknown		242	56.3%	626	71.1%	608	93.5%
	Yes	216	33.6%	30	5.2%	Unknown		188	43.7%	254	28.9%	42	6.5%
Charged but Acquitted	No	633	98.4%	577	100.0%	Unknown		402	93.5%	873	99.2%	644	99.1%
	Yes	10	1.6%	0	0.0%	Unknown		28	6.5%	7	0.8%	6	0.9%
Convicted	No	259	40.3%	479	83.0%	249	92.2%	216	50.2%	748	85.0%	530	81.5%
	Yes	384	59.7%	98	17.0%	21	7.8%	214	49.8%	132	15.0%	120	18.5%
Batterer Program	No	445	69.2%	577	100.0%	Unknown		331	77.0%	0	0.0%	650	100.0%
	Yes	198	30.8%	0	0.0%	Unknown		99	23.0%	880	100.0%	0	0.0%
Fine	No	604	93.9%	527	91.3%	267	98.9%	430	100.0%	0	0.0%	573	88.2%
	Yes	39	6.1%	50	8.7%	3	1.1%	0	0.0%	880	100.0%	77	11.8%
Probation	No	401	62.4%	568	98.4%	258	95.6%	331	77.0%	789	89.7%	609	93.7%
	Yes	242	37.6%	9	1.6%	12	4.4%	99	23.0%	91	10.3%	41	6.3%
Jail	No	450	70.0%	538	93.2%	260	96.3%	0	0.0%	845	96.0%	639	98.3%
	Yes	193	30.0%	39	6.8%	10	3.7%	0	0.0%	35	4.0%	11	1.7%

Table 3 - 2B Sanction Rates by the Studies

Sanctions		Studies											
		Jolin, et al.		Newmark, et al.		Orchowski		Weisz, et al.		Wilson & Klein		Wooldredge	
		N	%	N	%	N	%	N	%	N	%	N	%
Total Cases		927	100.0%	170	100.0%	1910	100.0%	1057	100.0%	342	100.0%	3662	100.0%
Arrest Warrant Issued	No	927	100.0%	170	100.0%	0	0.0%	1029	97.4%	258	75.4%	3662	100.0%
	Yes	0	0.0%	0	0.0%	1910	100.0%	28	2.6%	84	24.6%	0	0.0%
Arrested	No	0	0.0%	0	0.0%	0	0.0%	744	70.4%	119	34.8%	0	0.0%
	Yes	927	100.0%	170	100.0%	1910	100.0%	313	29.6%	223	65.2%	3662	100.0%
Charges Filed	No	595	64.2%	0	0.0%	0	0.0%	939	88.8%	145	42.4%	224	6.1%
	Yes	332	35.8%	170	100.0%	1910	100.0%	118	11.2%	197	57.6%	3438	93.9%
Charges Deferred	No	927	100.0%	170	100.0%	1910	100.0%	1057	100.0%	342	100.0%	3662	100.0%
	Yes	0	0.0%	0	0.0%	0	0.0%	0	0.0%	0	0.0%	0	0.0%
Charges Dismissed	No	803	86.6%	156	91.8%	1085	56.8%	1008	95.4%	234	68.4%	2013	55.0%
	Yes	124	13.4%	14	8.2%	825	43.2%	49	4.6%	108	31.6%	1649	45.0%
Charged but Acquitted	No	912	98.4%	163	95.9%	1766	92.5%	1052	99.5%	333	97.4%	3427	93.6%
	Yes	15	1.6%	7	4.1%	144	7.5%	5	0.5%	9	2.6%	235	6.4%
Convicted	No	734	79.2%	20	11.8%	921	48.2%	993	93.9%	188	55.0%	2108	57.6%
	Yes	193	20.8%	150	88.2%	989	51.8%	64	6.1%	154	45.0%	1554	42.4%
Batterer Program	No	806	86.9%	124	72.9%	1499	78.5%	1008	95.4%	342	100.0%	3416	93.3%
	Yes	121	13.1%	46	27.1%	411	21.5%	49	4.6%	0	0.0%	246	6.7%
Fine	No	782	84.4%	165	97.1%	1827	95.7%	1038	98.2%	337	98.5%	3662	100.0%
	Yes	145	15.6%	5	2.9%	83	4.3%	19	1.8%	5	1.5%	0	0.0%
Probation	No	783	84.5%	86	50.6%	1908	99.9%	1008	95.4%	277	81.0%	2637	72.0%
	Yes	144	15.5%	84	49.4%	2	0.1%	49	4.6%	65	19.0%	1025	28.0%
Jail	No	846	91.3%	73	42.9%	1433	75.0%	1043	98.7%	258	75.4%	3268	89.2%
	Yes	81	8.7%	97	57.1%	477	25.0%	14	1.3%	84	24.6%	394	10.8%

Table 3- 3 Most Severe Disposition by Study

	Most Severe Disposition								
	Not Charged		Charged						
	Not Convicted				Convicted				
Studies	Not Arrested	Arrested	Dismissed	Acquitted	Other	Fine	Program	Probation	Jail
Davis, et al.	0	0	249	10	4	36	1	150	193
Dunford	0	358	89	32	0	50	0	9	39
Fagan	179	14	56	0	0	2	0	9	10
Ford & Regoli	0	0	188	28	115	0	0	99	0
Hirschel, et al.	221	261	42	6	29	42	0	38	11
Jolin, et al.	0	595	124	15	29	4	4	75	81
Newmark, et al.	0	0	12	7	17	1	4	32	97
Orchowski	0	0	803	118	132	52	328	0	477
Weisz, et al.	688	251	49	5	5	0	0	45	14
Wilson & Klein	8	63	108	9	0	5	0	65	84
Wooldredge	0	224	1649	235	0	0	246	914	394

	Most Severe Disposition								
	Not Charged		Charged						
	Not Convicted				Convicted				
Studies	Not Arrested	Arrested	Dismissed	Acquitted	Other	Fine	Program	Probation	Jail
Davis, et al.	0%	0%	39%	2%	1%	6%	0%	23%	30%
Dunford	0%	62%	15%	6%	0%	9%	0%	2%	7%
Fagan	66%	5%	21%	0%	0%	1%	0%	3%	4%
Ford & Regoli	0%	0%	44%	7%	27%	0%	0%	23%	0%
Friday	43%	13%	29%	1%	1%	0%	0%	10%	4%
Hirschel, et al.	34%	40%	6%	1%	4%	6%	0%	6%	2%
Jolin, et al.	0%	64%	13%	2%	3%	0%	0%	8%	9%
Newmark, et al.	0%	0%	7%	4%	10%	1%	2%	19%	57%
Orchowski	0%	0%	42%	6%	7%	3%	17%	0%	25%
Weisz, et al.	65%	24%	5%	0%	0%	0%	0%	4%	1%
Wilson & Klein	2%	18%	32%	3%	0%	1%	0%	19%	25%
Wooldredge	0%	6%	45%	6%	0%	0%	7%	25%	11%

Table 3 - 4: Hypotheses about Crime Control Effects

Prosecution Hypotheses

- **Prosecution given offense**
- **Posecution given arrest**

Conviction Hypotheses

- **Conviction given offense**
- **Conviction given arrest**
- **Conviction given prosecution**

Sentence Severity Hypotheses

- **Jail given offense**
- **Jail given arrest**
- **Jail given prosecution**
- **Jail given conviction**

- **Severity given offense**
- **Severity given arrest**
- **Severity given prosecution**
- **Severity given conviction**

Table 3 - 5: Outcome Measures in Published Research and in Available Data (p. 1 of 4)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Victim	Type of Offense	Criteria for Repeat	Months At Risk	Sample Size	Base Rate
Davis, et al., 1998								
Published	Police Records	Disposition	Any	Any	Arrest	6	1,133	N.R.
Available Data	Police Records	Disposition	Any	Any	Offense	6	643	17.9%
			Any	Any	Arrest	6	643	6.2%
Dunford, et al, 1990								
Published	Police Reports	Offense	Same	Any	Arrest	6	330	10.6%
			Same	Any	Offense	6		16.7%
Dunford, 1990								
Published	Police Records	Offense	Same	Any	Arrest	6	247	8.9%
			Same	Any	Offense	6		18.6%
			Same	Any	Arrest	12		16.2%
			Same	Any	Offense	12		28.7%
Dunford, 1990; Dunford, et al. 1990								
Available Data	Police Records	Offense	Any	Any	Arrest	6	577	12.3%
			Same	Any	Arrest	6		6.9%
			Any	Violent	Arrest	6		7.6%
			Same	Violent	Arrest	6		5.5%
			Any	Any	Offense	6		15.6%
			Same	Any	Offense	6		10.7%
			Any	Violent	Offense	6		9.4%
			Same	Violent	Offense	6		7.6%
			Any	Any	Arrest	12	577	20.6%
			Same	Any	Arrest	12		13.9%
			Any	Violent	Arrest	12		13.0%
			Same	Violent	Arrest	12		10.2%
			Any	Any	Offense	12		23.1%
			Same	Any	Offense	12		16.6%
			Any	Violent	Offense	12		15.1%
			Same	Violent	Offense	12		12.5%

Table 3 - 5: Outcome Measures in Published Research and in Available Data (Page 2 of 4)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Victim	Type of Offense	Criteria for Repeat	Months At Risk	Sample Size	Base Rate
Fagan, 1989								
Published	Victim Interviews	Offense	Same	Violence	Offense	6	270	28.5%
Available Data	Victim Interviews	Offense	Same	Violence	Offense	6	270	28.5%
Ford & Regoli, 1992b								
Published	Victim Interviews	Disposition	Same	Violence	Offense	6	430	30.9%
Available Data	Victim Interviews	Disposition	Same	Violence	Offense	6	430	30.9%
Friday, et al., 2006								
Published	Police Records	Offense	Same	Violence	Offense	24	790	34.1%
Available Data	Police Records	Offense	Any	Any	Offense	24	880	24.8%
			Any	Violence	Offense	24		21.0%
			Same	Violence	Offense	24		16.6%
Hirschel, et al., 1990								
Published	Police Record	Initiation	Same	Any	Arrest	6	650	18.2%
Available Data	Police Record	Disposition	Any	Any	Arrest	6	521	32.1%
			Same	Any	Arrest			16.5%
			Any	Violence	Arrest			13.1%
			Same	Violence	Arrest			8.8%

Table 3 - 5: Outcome Measures in Published Research and in Available Data (Page 3 of 4)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Victim	Type of Offense	Criteria for Repeat	Months At Risk	Sample Size	Base Rate
Jolin, et al., 1998								
Published	Records on Victim	Offense	Same	Any	Offense	6	883	14.0%
			Same	Any	Arrest	6		7.8%
	Records on Offender		Same	Any	Offense	6		13.6%
			Same	Any	Arrest	6		8.0%
	Police Records		Any	Any	Dispatch	6		46.0%
			Same	Any	Dispatch	6		14.0%
Available Data	Police Records	Offense	Any	Any	Offense	6	927	15.5%
			Same	Any	Offense	6		12.1%
			Same	Violent	Offense	6		6.1%
Newmark, et al., 2001								
Published	Police Records	Disposition	Any	Any	Arrest	12	304	31.3%
			Any	Felony Violence	Arrest	12		4.9%
			Any	Contempt	Arrest	12		11.1%
			Any	Any	Arrest	18		39.9%
			Any	Felony Violence	Arrest	18		7.2%
			Any	Contempt	Arrest	18		15.2%
Available Data	Police Records	Disposition	Any	Any	Arrest	12	170	26.5%
			Any	Felony Violence	Arrest	12	170	5.9%
			Any	Contempt	Arrest	12	170	7.1%
			Any	Any	Arrest	18	152	33.6%
			Any	Felony Violence	Arrest	18	152	7.9%
			Any	Contempt	Arrest	18	152	10.5%

Table 3 - 5: Outcome Measures in Published Research and in Available Data (Page 4 of 4)

Report Author/Date	Data Source	Time at Risk Starts at	Type of Victim	Type of Offense	Criteria for Repeat	Months At Risk	Sample Size	Base Rate
Orchowsky, 1999								
Published	Police Records	Offense	Same	Violence	Arrest	variable	1910	21.0%
Available Data	Police Records	Disposition	Same	Violence	Arrest	6	1556	10.0%
			Same	Violence	Arrest	12	1297	14.5%
Weisz, et al., 2001								
Published	Police Records	Offense	Any	Any	Offense	12	1057	11.4%
Available Data	Police Records	Offense	Any	Any	Offense	12	1050	11.4%
			Same	Any	Offense	12	1050	8.9%
Wilson & Klein, 2006								
Published	Police Records	Offense	Any	Any	Arrest	108	342	65.5%
			Same	Violence	Arrest			50.9%
			Same	Violence/RO	Arrest			59.9%
Data Available	Police Records	Disposition	Any	Any	Arrest	6	342	11.7%
			Any	Violence	Arrest	6		3.8%
			Any	Any	Arrest	12		23.1%
			Any	Violence	Arrest	12		7.3%
			Any	Any	Arrest	108		57.0%
			Any	Violence	Arrest	108		37.4%
Wooldredge & Thistlethwaite, 1999; 2002; 2005								
Published	Police Records	Completion	Same	Violence	Arrest	24	3110	14.4%
Available Data	Police Records	Completion	Same	Violence	Arrest	24	3110	14.4%

Table 3 - 6 Case Characteristics by Study (Page 1 of 2)

Incident Characteristics		Davis, et al.		Dunford		Fagan		Ford & Regoli		Friday		Hirschel, et al.	
		N	%	N	%	N	%	N	%	N	%	N	%
Total Sample		643	100.0%	577	100.0%	270	100.0%	430	100.0%	880	100.0%	650	100.0%
Male Victim	No	643	100.0%	553	95.8%	270	100.0%	430	100.0%	734	83.4%	650	100.0%
	Yes	0	0.0%	24	4.2%	0	0.0%	0	0.0%	145	16.5%	0	0.0%
	Missing	0	0.0%	0	0.0%	0	0.0%	0	0.0%	1	0.1%	0	0.0%
Male Offender	No	69	10.7%	16	2.8%	16	5.9%	0	0.0%	126	14.3%	0	0.0%
	Yes	574	89.3%	539	93.4%	254	94.1%	430	100.0%	747	84.9%	650	100.0%
	Missing	0	0.0%	22	3.8%	0	0.0%	0	0.0%	7	0.8%	0	0.0%
Victim White	No	0	0.0%	265	45.9%	115	42.6%	190	44.2%	591	67.2%	453	69.7%
	Yes	0	0.0%	312	54.1%	155	57.4%	240	55.8%	285	32.4%	193	29.7%
	Missing	643	100.0%	0	0.0%	0	0.0%	0	0.0%	4	0.5%	4	0.6%
Offender White	No	0	0.0%	301	52.2%	122	45.2%	193	44.9%	629	71.5%	472	72.6%
	Yes	0	0.0%	276	47.8%	148	54.8%	237	55.1%	246	28.0%	178	27.4%
	Missing	643	100.0%	0	0.0%	0	0.0%	0	0.0%	5	0.6%	0	0.0%
Children in Family	No	0	0.0%	95	16.5%	55	20.4%	167	38.8%	114	13.0%	385	59.2%
	Yes	0	0.0%	340	58.9%	215	79.6%	263	61.2%	290	33.0%	247	38.0%
	Missing	643	100.0%	142	24.6%	0	0.0%	0	0.0%	476	54.1%	18	2.8%
Offender Employed	No	0	0.0%	143	24.8%	78	28.9%	111	25.8%	0	0.0%	143	22.0%
	Yes	0	0.0%	305	52.9%	181	67.0%	319	74.2%	0	0.0%	446	68.6%
	Missing	643	100.0%	129	22.4%	11	4.1%	0	0.0%	880	100.0%	61	9.4%
Married	No	499	77.6%	334	57.9%	0	0.0%	285	66.3%	619	70.3%	339	52.2%
	Yes	144	22.4%	243	42.1%	0	0.0%	145	33.7%	170	19.3%	311	47.8%
	Missing	0	0.0%	0	0.0%	270	100.0%	0	0.0%	91	10.3%	0	0.0%
Intimate Partners	No	105	16.3%	42	7.3%	45	16.7%	0	0.0%	167	19.0%	0	0.0%
	Yes	538	83.7%	535	92.7%	225	83.3%	430	100.0%	622	70.7%	650	100.0%
	Missing	0	0.0%	0	0.0%	0	0.0%	0	0.0%	91	10.3%	0	0.0%
Live Together	No	0	0.0%	145	25.1%	151	55.9%	191	44.4%	388	44.1%	91	14.0%
	Yes	0	0.0%	325	56.3%	103	38.1%	239	55.6%	389	44.2%	559	86.0%
	Missing	643	100.0%	107	18.5%	16	5.9%	0	0.0%	103	11.7%	0	0.0%
Suspect Using Alcohol	No	0	0.0%	248	43.0%	0	0.0%	0	0.0%	72	8.2%	294	45.2%
	Yes	0	0.0%	310	53.7%	0	0.0%	0	0.0%	61	6.9%	343	52.8%
	Missing	643	100.0%	19	3.3%	270	100.0%	430	100.0%	747	84.9%	13	2.0%
Victim Injured	No	232	36.1%	133	23.1%	87	32.2%	0	0.0%	291	33.1%	118	18.2%
	Yes	411	63.9%	444	76.9%	180	66.7%	0	0.0%	472	53.6%	525	80.8%
	Missing	0	0.0%	0	0.0%	3	1.1%	430	100.0%	117	13.3%	7	1.1%
Felony Charges	No	643	100.0%	577	100.0%	0	0.0%	430	100.0%	391	44.4%	650	100.0%
	Yes	0	0.0%	0	0.0%	0	0.0%	0	0.0%	9	1.0%	0	0.0%
	Missing	0	0.0%	0	0.0%	270	100.0%	0	0.0%	480	54.5%	0	0.0%
Offender Prior Arrest	No	410	63.8%	214	37.1%	166	61.5%	103	24.0%	272	30.9%	450	69.2%
	Yes	233	36.2%	363	62.9%	104	38.5%	327	76.0%	576	65.5%	200	30.8%
	Missing	0	0.0%	0	0.0%	0	0.0%	0	0.0%	32	3.6%	0	0.0%
Average Age of Victim	Mean	NA		29.65	81.5%	31.1	100.0%	28.5	99.3%	33.3	95.1%	30.4	98.6%
	Missing	643	100.0%	107	18.5%	0	0.0%	3	0.7%	43	4.9%	9	1.4%
Average Age of Offender	Mean	NA		30.66	81.3%	33.4	99.3%	30.7	100.0%	31.8	98.3%	32.7	99.5%
	Missing	643	100.0%	108	18.7%	2	0.7%	0	0.0%	15	1.7%	3	0.5%

Table 3 - 6 Case Characteristics by Study (page 2 of 2)

Incident Characteristics		Joiin, et al.		Newmark		Orchowsky		Weisz, et al.		Wilson & Klein		Wooldredge	
		N	%	N	%	N	%	N	%	N	%	N	%
Total Sample		927	100.0%	170	100.0%	1910	100.0%	1057	100.0%	342	100.0%	3662	100.0%
Male Victim	No	0	0.0%	157	27%	0	0.0%	1057	245.8%	342	38.9%	3662	563.4%
	Yes	0	0.0%	13	2%	0	0.0%	0	0.0%	0	0.0%	0	0.0%
	Missing	927	144.2%	0	0%	1910	707.4%	0	0.0%	0	0.0%	0	0.0%
Male Offender	No	927	144.2%	8	1%	0	0.0%	0	0.0%	0	0.0%	585	90.0%
	Yes	0	0.0%	162	28%	1910	707.4%	1057	245.8%	342	38.9%	3077	473.4%
	Missing	0	0.0%	0	0%	0	0.0%	0	0.0%	0	0.0%	0	0.0%
Victim White	No	256	39.8%	134	23%	0	0.0%	991	230.5%	19	2.2%	0	0.0%
	Yes	670	104.2%	28	5%	0	0.0%	36	8.4%	125	14.2%	0	0.0%
	Missing	1	0.2%	8	1%	1910	707.4%	30	7.0%	198	22.5%	3662	563.4%
Offender White	No	350	54.4%	141	24%	1414	523.7%	996	231.6%	47	5.3%	2173	334.3%
	Yes	577	89.7%	29	5%	496	183.7%	23	5.3%	275	31.3%	1489	229.1%
	Missing	0	0.0%	0	0%	0	0.0%	38	8.8%	20	2.3%	0	0.0%
Children in Family	No	80	12.4%	18	3%	1578	584.4%	586	136.3%	0	0.0%	721	110.9%
	Yes	389	60.5%	103	18%	332	123.0%	455	105.8%	0	0.0%	2941	452.5%
	Missing	458	71.2%	49	8%	0	0.0%	16	3.7%	342	38.9%	0	0.0%
Offender Employed	No	205	31.9%	79	14%	761	281.9%	0	0.0%	61	6.9%	1693	260.5%
	Yes	263	40.9%	87	15%	1149	425.6%	0	0.0%	189	21.5%	1969	302.9%
	Missing	459	71.4%	4	1%	0	0.0%	1057	245.8%	92	10.5%	0	0.0%
Married	No	304	47.3%	130	23%	1339	495.9%	807	187.7%	234	26.6%	2453	377.4%
	Yes	173	26.9%	40	7%	571	211.5%	234	54.4%	78	8.9%	1209	186.0%
	Missing	450	70.0%	0	0%	0	0.0%	16	3.7%	30	3.4%	0	0.0%
Intimate Partners	No	477	74.2%	16	3%	0	0.0%	0	0.0%	78	8.9%	0	0.0%
	Yes	450	70.0%	154	27%	1910	707.4%	1057	245.8%	234	26.6%	3662	563.4%
	Missing	0	0.0%	0	0%	0	0.0%	0	0.0%	30	3.4%	0	0.0%
Live Together	No	152	23.6%	83	14%	1910	707.4%	394	91.6%	0	0.0%	1049	161.4%
	Yes	325	50.5%	87	15%	0	0.0%	647	150.5%	0	0.0%	2613	402.0%
	Missing	450	70.0%	0	0%	0	0.0%	16	3.7%	342	38.9%	0	0.0%
Suspect Using Alcohol	No	6	0.9%	0	0%	1418	525.2%	637	148.1%	21	2.4%	0	0.0%
	Yes	395	61.4%	0	0%	492	182.2%	274	63.7%	103	11.7%	0	0.0%
	Missing	526	81.8%	170	29%	0	0.0%	146	34.0%	218	24.8%	3662	563.4%
Victim Injured	No	52	8.1%	51	9%	1096	405.9%	557	129.5%	187	21.3%	0	0.0%
	Yes	548	85.2%	119	21%	814	301.5%	484	112.6%	106	12.0%	0	0.0%
	Missing	327	50.9%	0	0%	0	0.0%	16	3.7%	49	5.6%	3662	563.4%
Felony Charges	No	888	138.1%	170	0%	1787	661.9%	102	23.7%	342	38.9%	3662	563.4%
	Yes	39	6.1%	0	100%	123	45.6%	45	10.5%	0	0.0%	0	0.0%
	Missing	0	0.0%	0	0%	0	0.0%	910	211.6%	0	0.0%	0	0.0%
Offender Prior Arrest	No	477	74.2%	70	12%	0	0.0%	277	64.4%	52	5.9%	2414	371.4%
	Yes	450	70.0%	100	17%	0	0.0%	487	113.3%	285	32.4%	1248	192.0%
	Missing	0	0.0%	0	0%	1910	707.4%	293	68.1%	5	0.6%	0	0.0%
Average Age of Victim	Mean	32.2	5.0%	33.57	96%	31.7	100.0%	30.2	87.4%	33.8	98.6%	N.A.	
	Missing	546	84.9%	24	4%	0	0.0%	54	12.6%	12	1.4%	3662	100.0%
Average Age of Offender	Mean	N.A.		34.18	100%	NA		32.4	84.2%	34.3	87.7%	32.0	100.0%
	Missing	927	100.0%	0	0%	1910	707.4%	68	15.8%	108	12.3%	0	0.0%

Table 3 - 7
Effectiveness of Sanctions for Intimate Partner Violence From Official Records Using Prevalence Measures

Research Location	Prosecution			Conviction		
	Less	None	More	Less	None	More
Alexandria, VA, 1993 - 96	0	4	0	0	4	0
Brooklyn, NY, 1997	0	0	0	0	6	0
Charlotte, 1987-88	0	8	0	0	8	0
Charlotte, 2003	0	6	0	0	6	0
Cincinnati, 1995	2	0	0	2	0	0
Detroit, 1998	0	4	0	0	4	0
Five Sites, 1980	0	2	0	0	4	0
Marion County, IN, 1987	0	0	0	0	2	0
Milwaukee, WI, 1994	0	0	0	0	8	0
Omaha, NE, 1986	0	64	0	0	64	0
Portland, OR, 1996	0	5	1	0	2	4
Quincy, MA, 1995	0	7	5	0	4	8
Totals	2	100	6	2	112	12

Research Location	Jail			Sentence Severity		
	Less	None	More	Less	None	More
Alexandria, VA, 1993 - 96	0	0	0	0	0	0
Brooklyn, NY, 1997	0	6	0	0	0	0
Charlotte, 1987-88	0	8	0	0	8	0
Charlotte, 2003	0	6	0	0	6	0
Cincinnati, 1995	0	0	0	0	0	6
Detroit, 1998	0	1	3	0	0	0
Five Sites, 1980	0	2	2	0	2	0
Marion County, IN, 1987	0	0	0	0	2	0
Milwaukee, WI, 1994	0	3	1	0	8	0
Omaha, NE, 1986	0	20	12	0	15	1
Portland, OR, 1996	0	2	4	0	3	3
Quincy, MA, 1995	0	4	8	0	0	0
Totals	0	52	30	0	44	10

Figure 3 - 1: Sample Sizes in 12 Data Collections

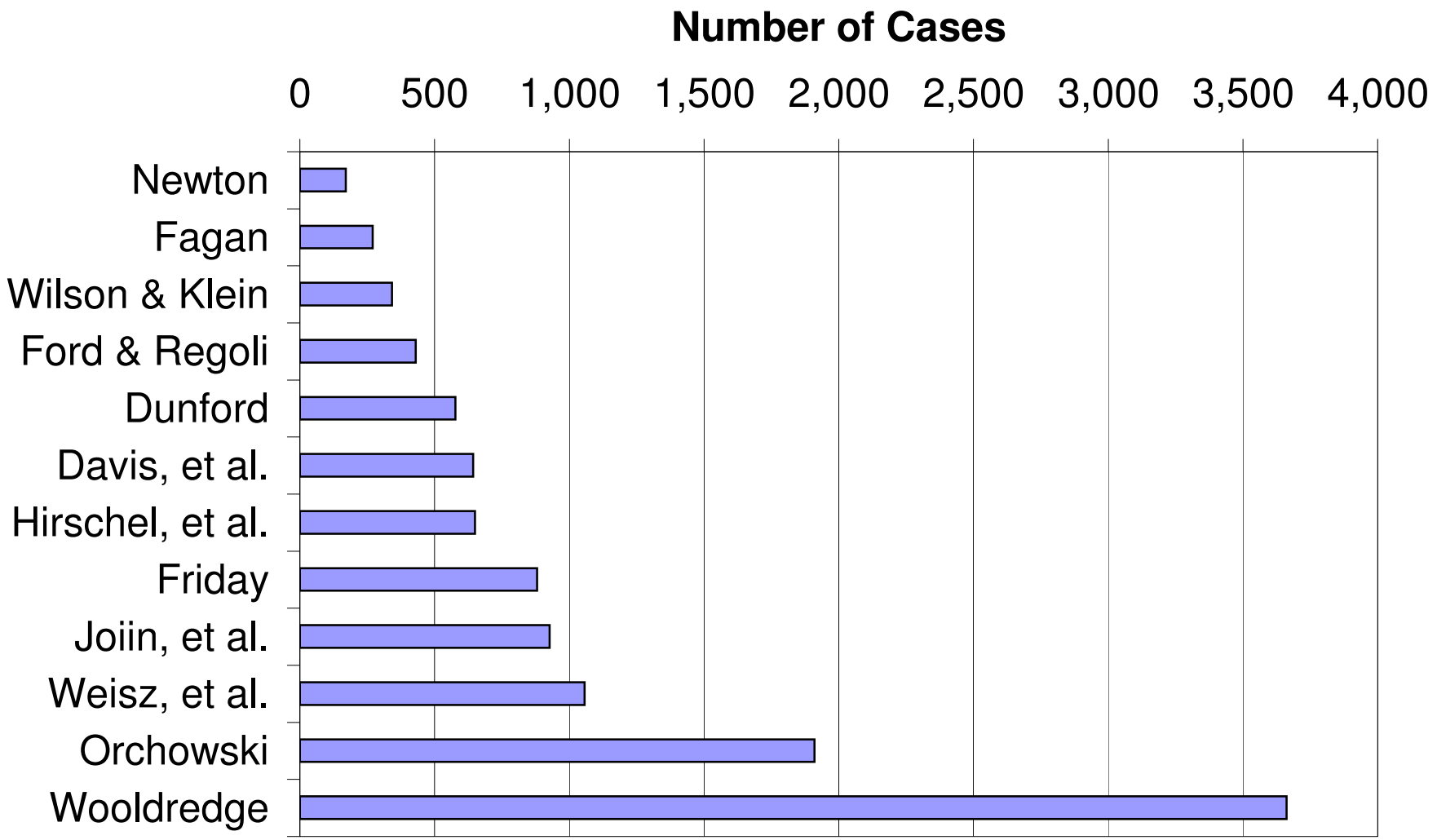


Figure 3 - 2: Sanctions Imposed
Percent Arrested, Charged, Convicted and Jailed

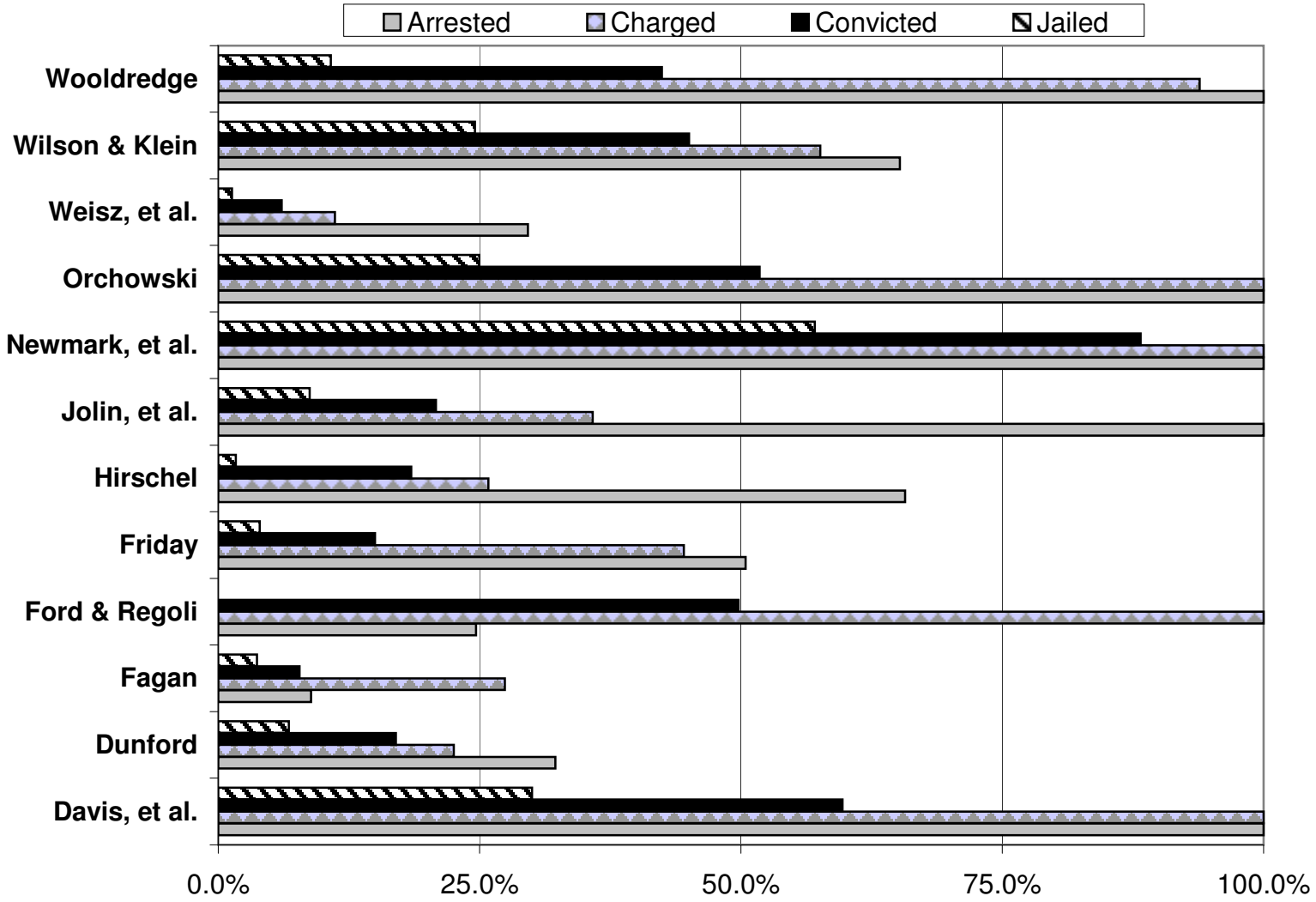
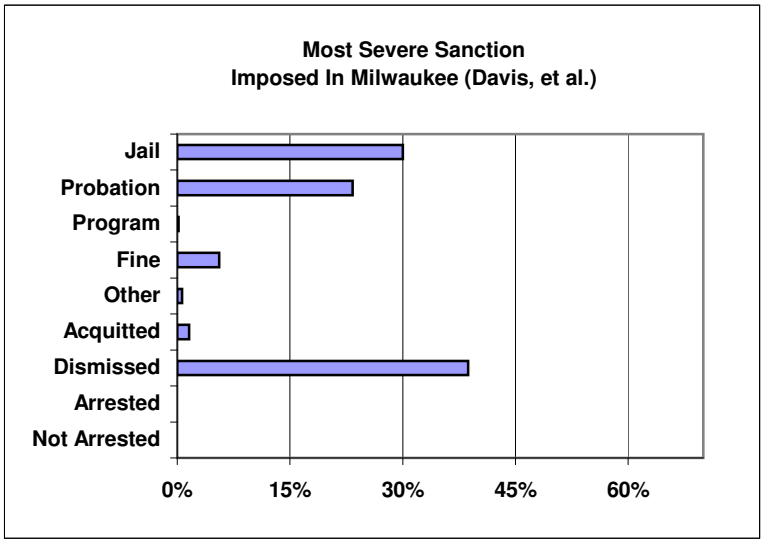
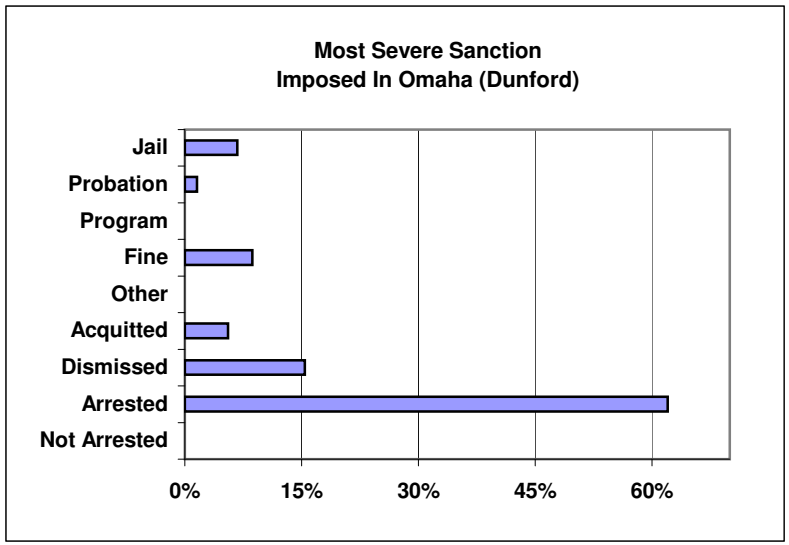
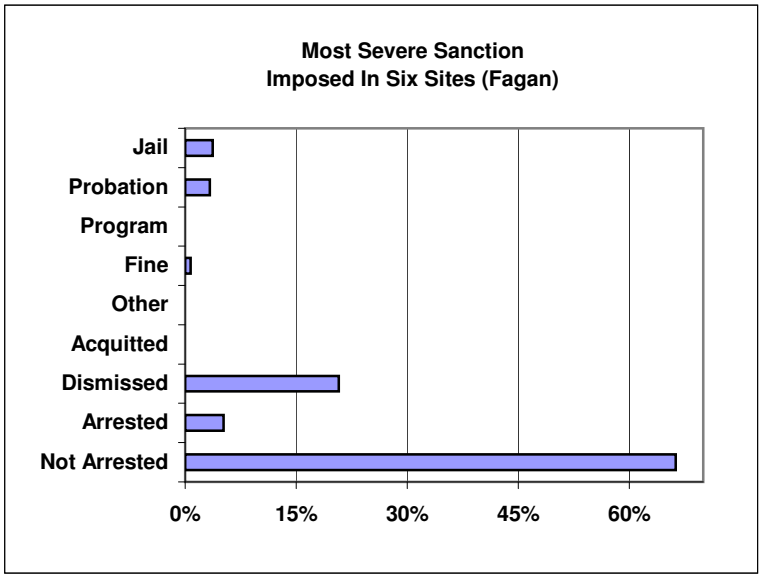


Figure 3 - 3A: Most Severe Sanctions Imposed by Study







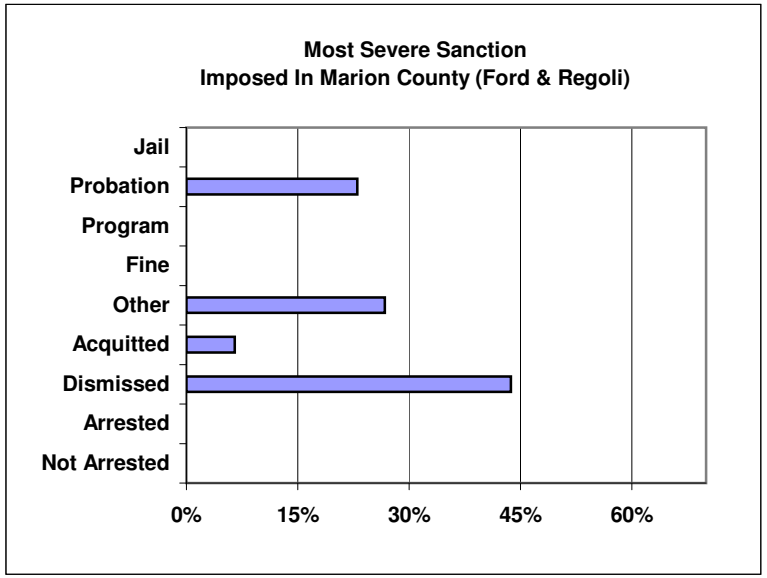
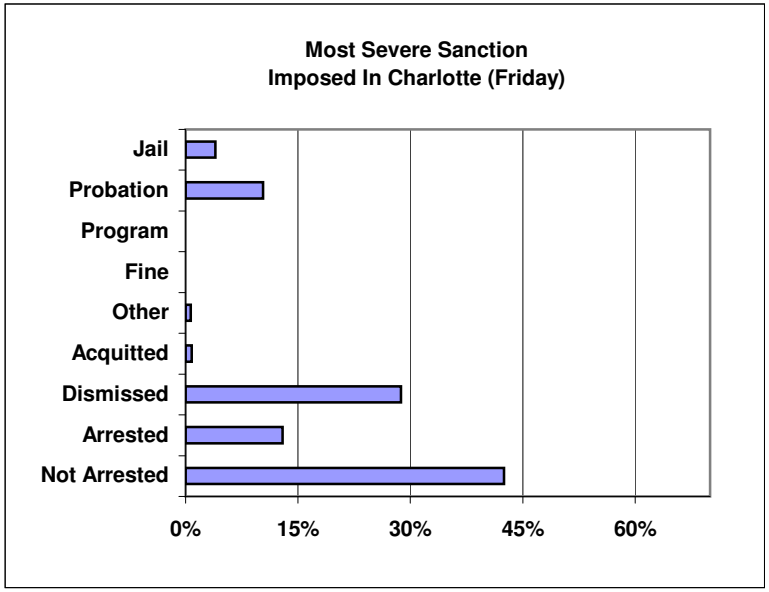
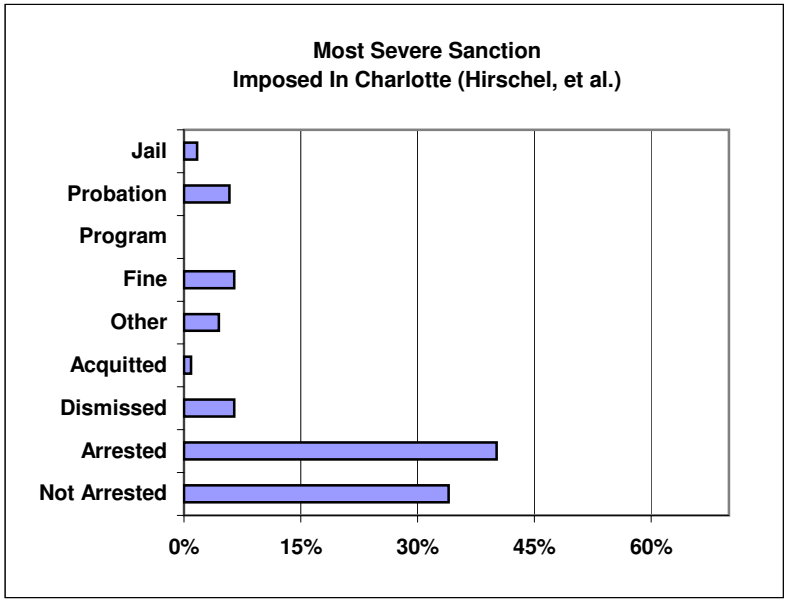
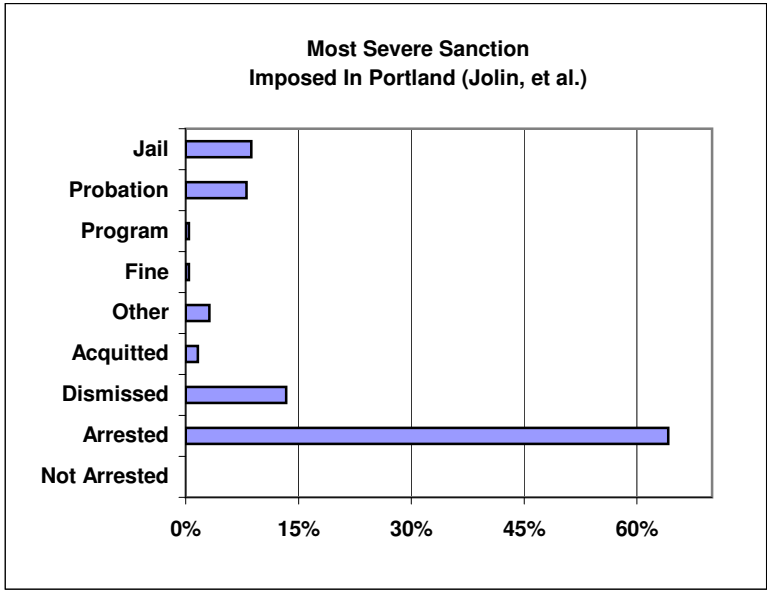


Figure 3 - 3B: Most Severe Sanctions Imposed by Study







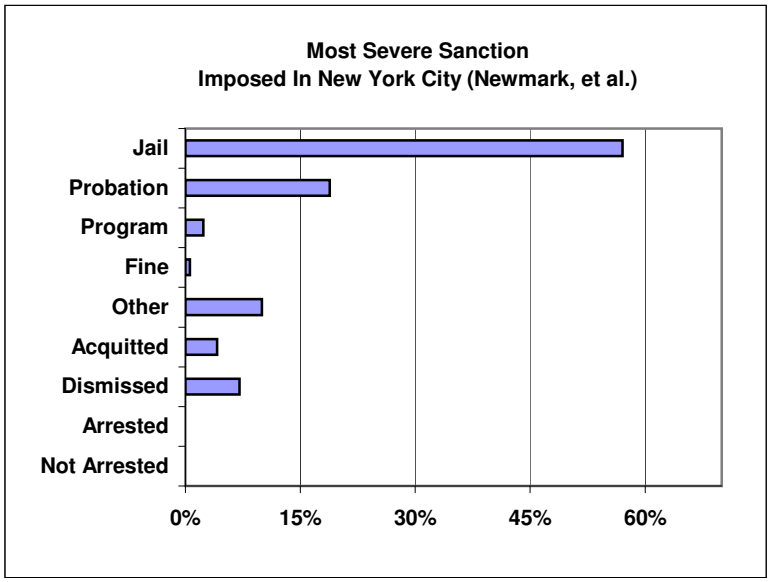
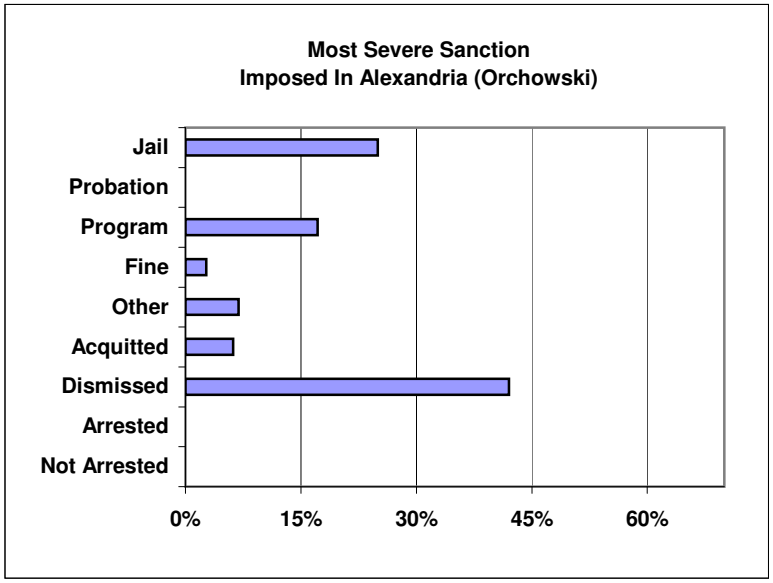
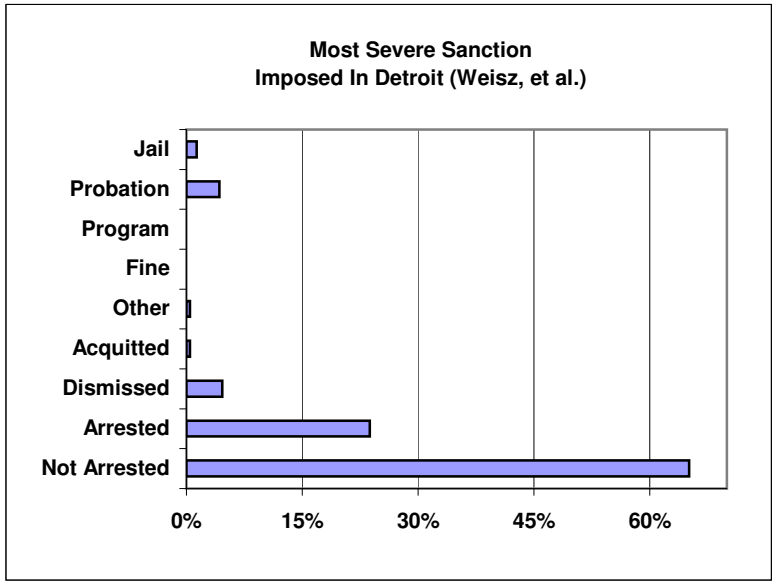
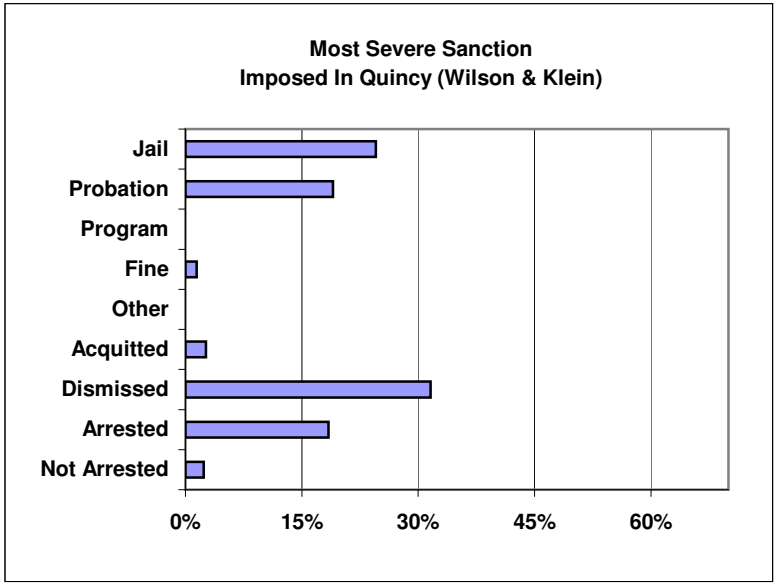


Figure 3 - 3C: Most Severe Sanctions Imposed by Study







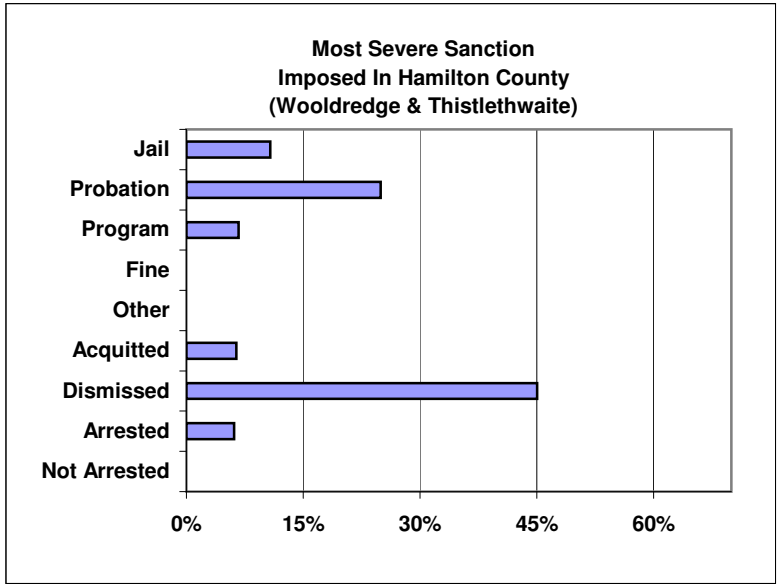
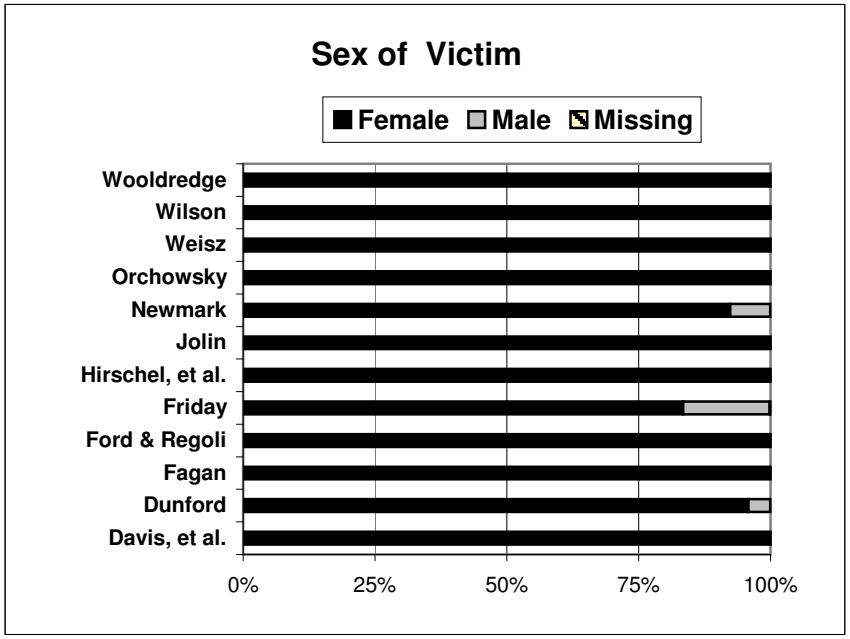
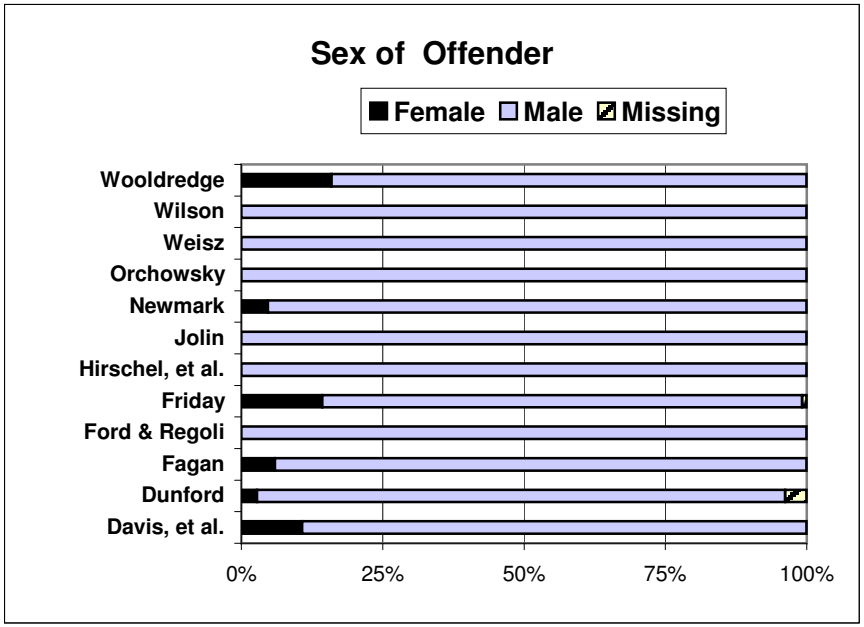
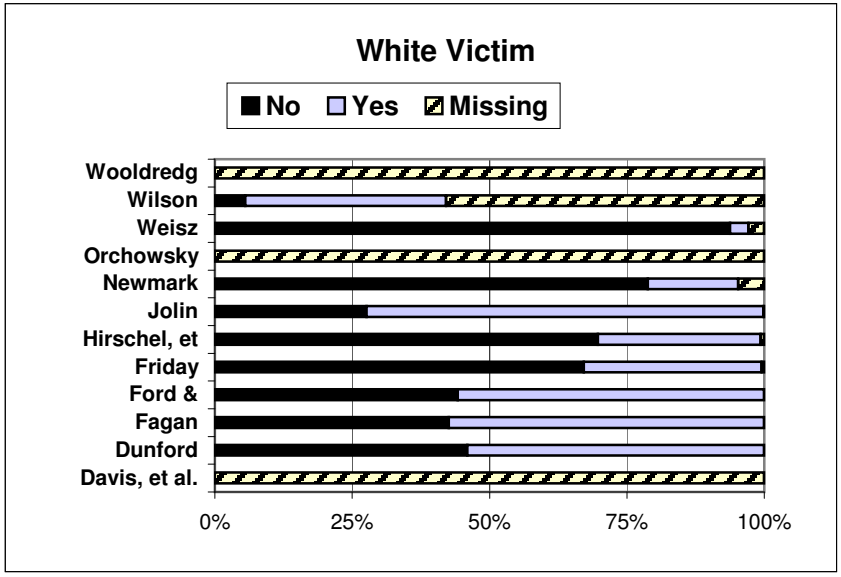


Figure 3 - 4A Case Characteristics by Study







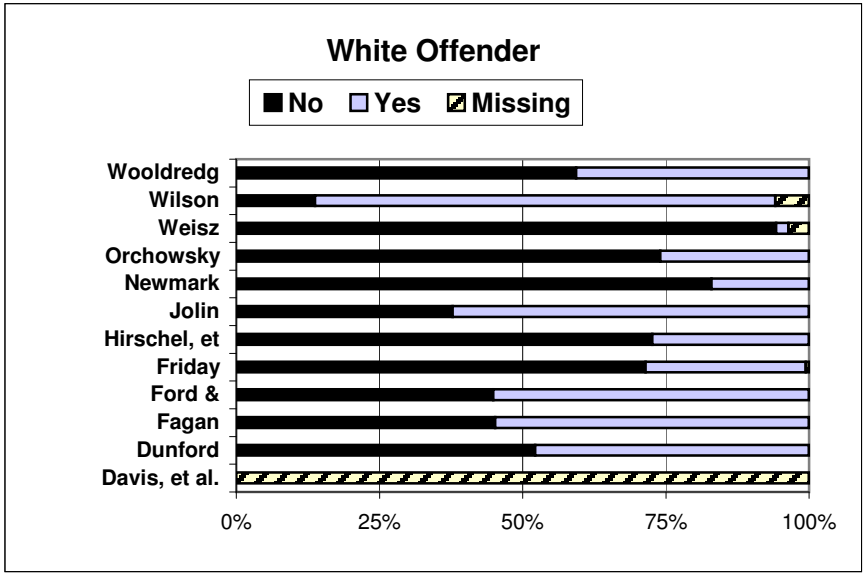
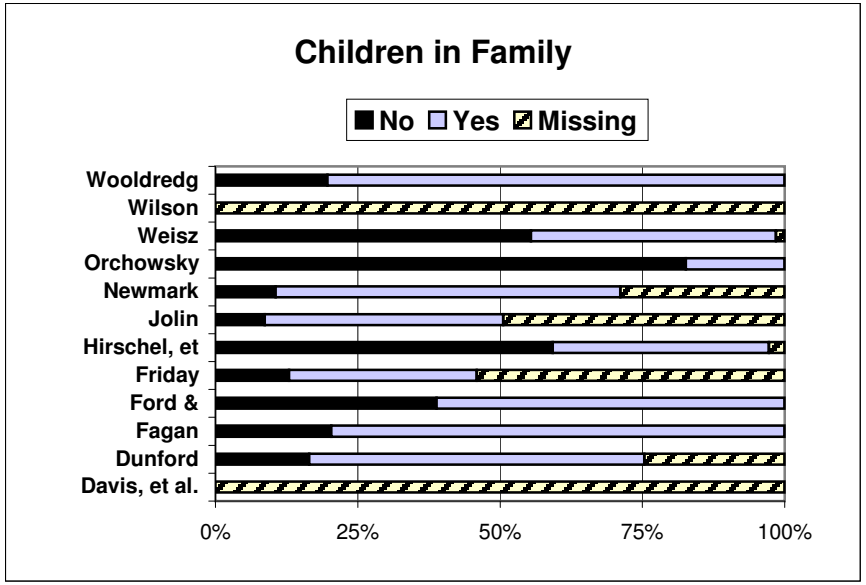
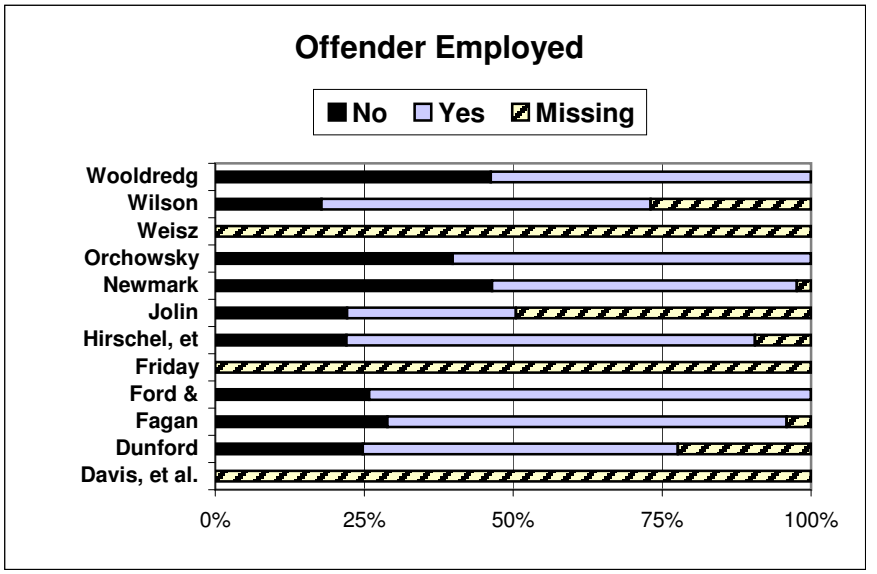
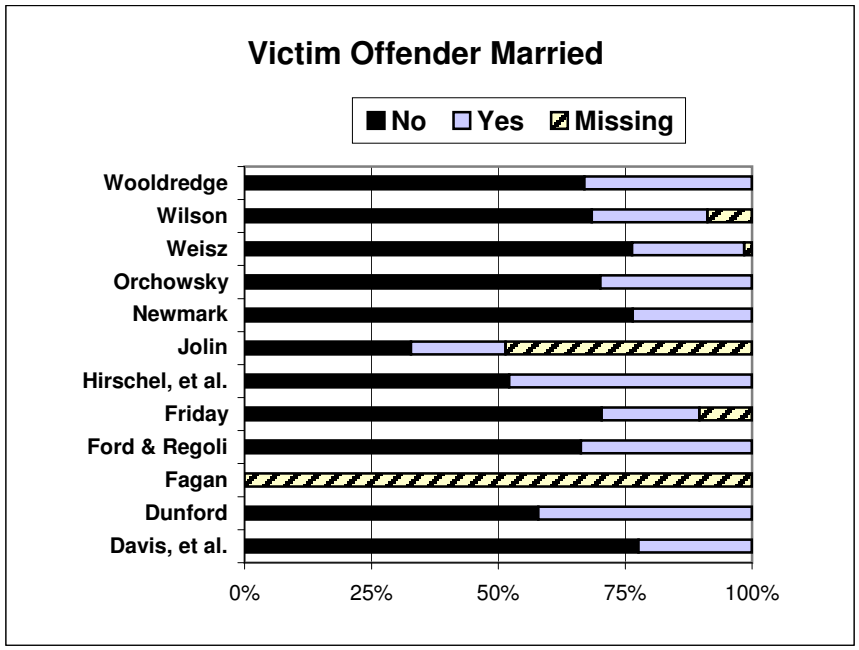


Figure 3 - 4B Case Characteristics by Study







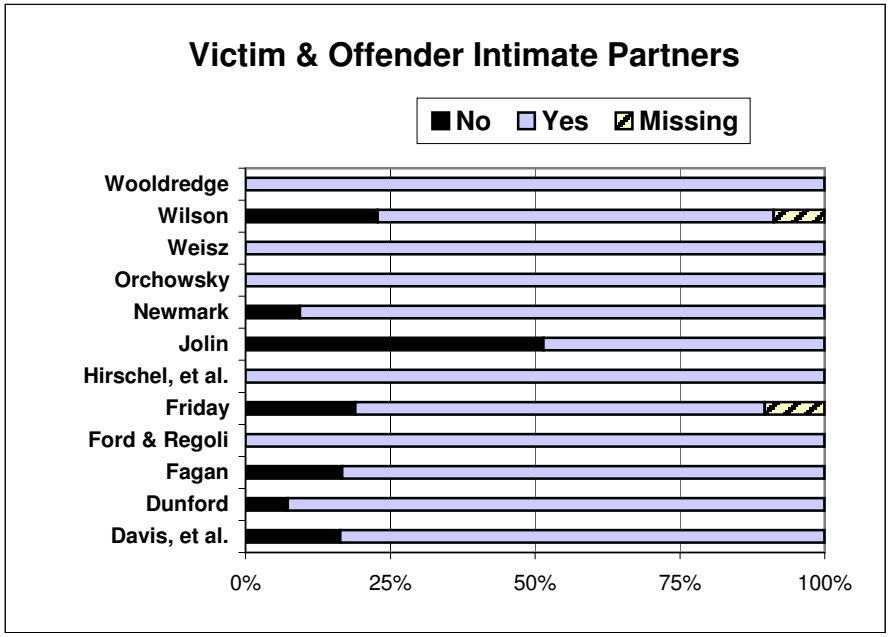
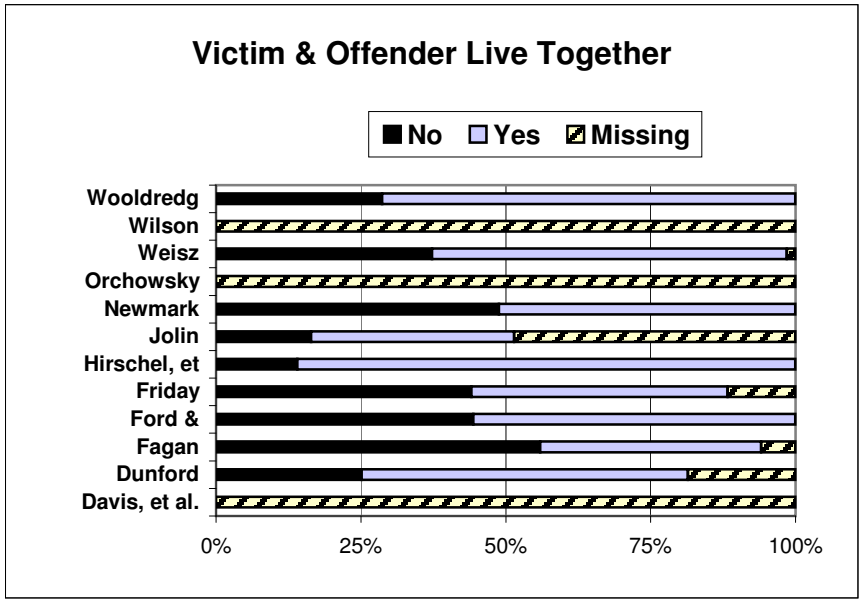
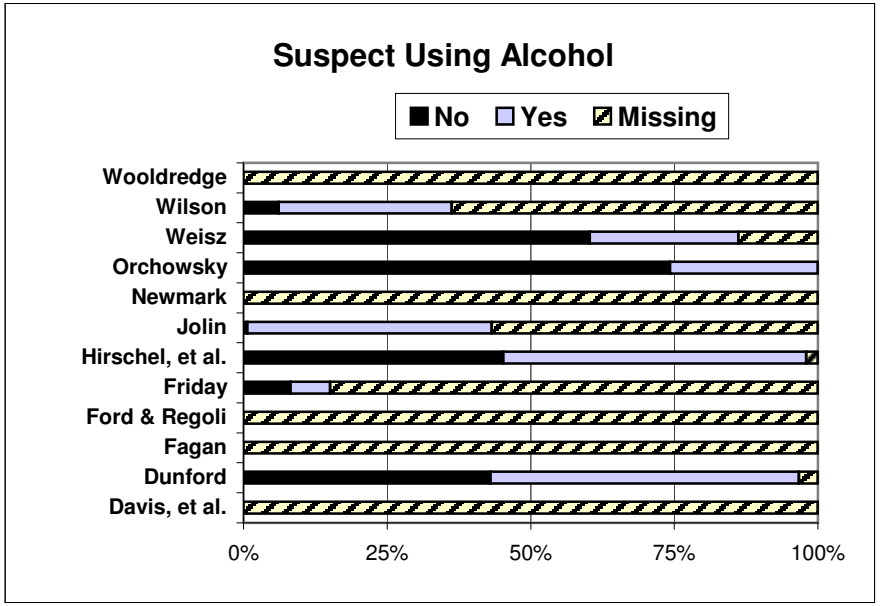
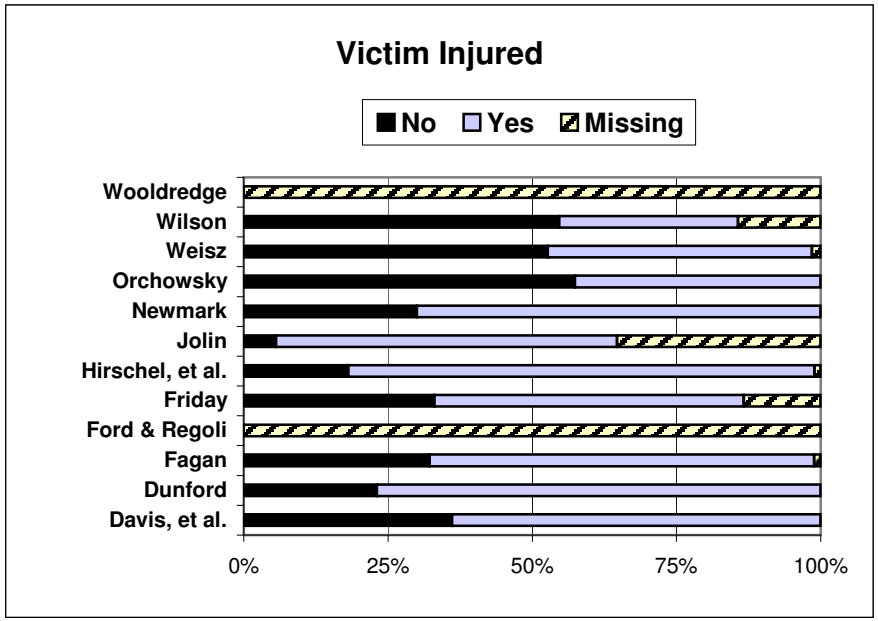


Figure 3 - 4C Case Characteristics by Study







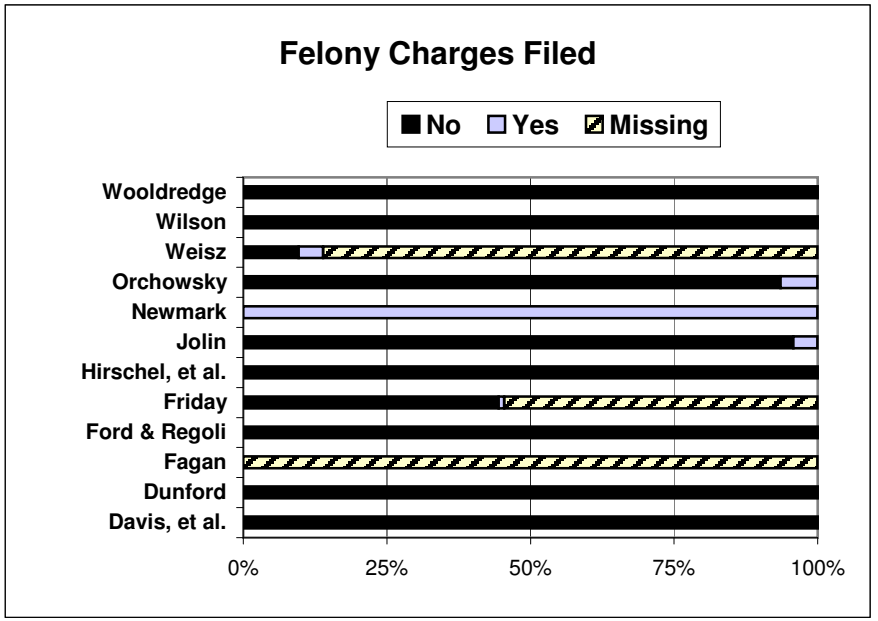
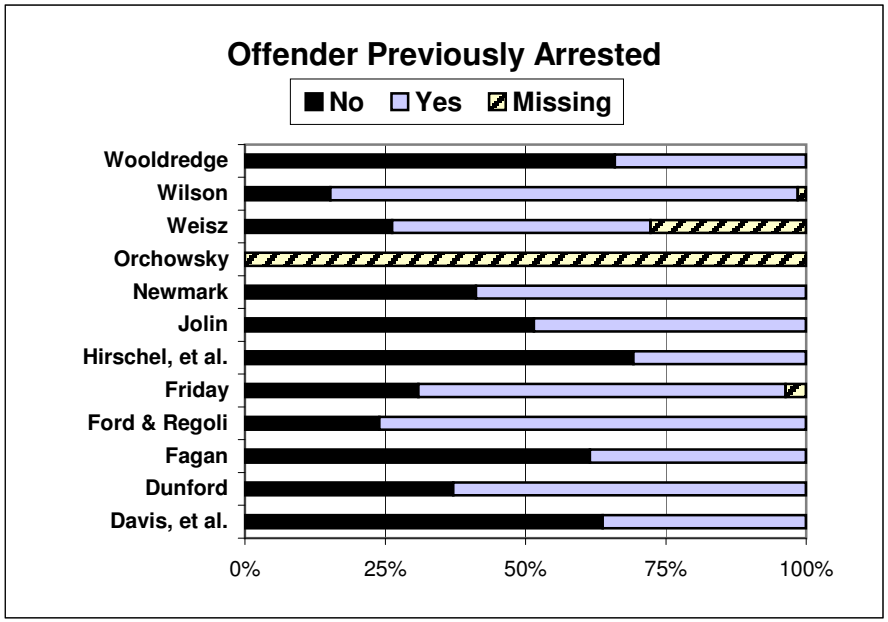
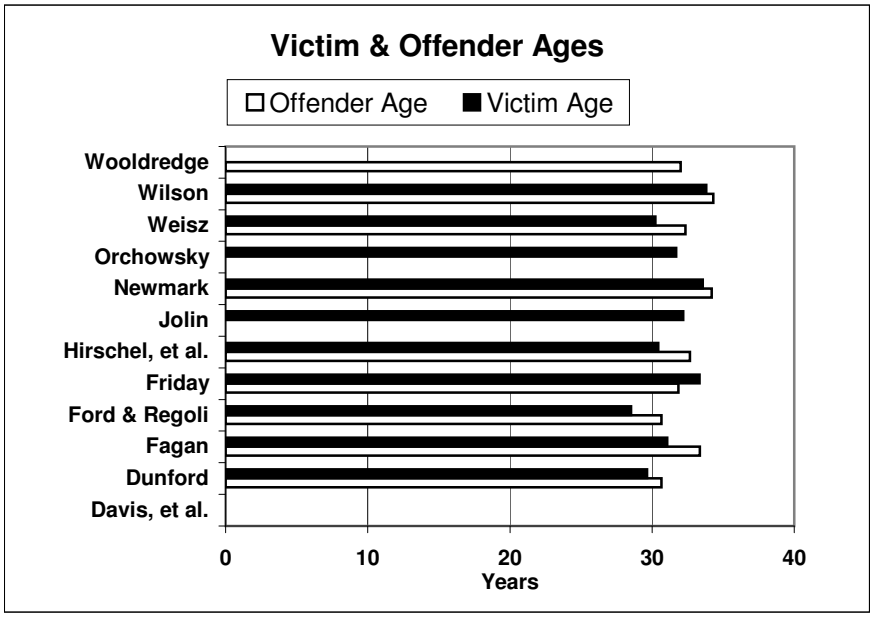


Figure 3 - 4D Case Characteristics by Study





Appendix 1: Rate of Prosecutions Per Reported Intimate Partner Violence Offense

Authors	Jurisdictions	Data Type	Offenses	Prosecutions	Prosecutions Per Offense
Field & Field 1973	Washington, D. C.	Prospective	7,500	199	2.7
Cook, et al. 2004	Five British Sites	Prospective	6,633	216	3.3
Kelley & O'Brien 1994	Boston	Prospective	2,152	107	5.0
Hanmer, et al. 1999	West Yorkshire	Prospective	1,870	139	7.4
Ford 1983	Marion Co.	Prospective	325	30	9.2
McLeod 1983	Detroit	Prospective	5,480	515	9.4
Schmidt & Steury 1989	Milwaukee	Prospective	2,212	209	9.4
Walby & Allen 2004	Britain	Interview	148	16	11.0
Greenwood, et al. 1973	Los Angeles	Prospective	524	62	11.8
Stroshine & Robinson 2003	Midwest U.S. City	Prospective	219	26	11.9
Weisz, et al. 2001	Detroit	Prospective	1,057	149	14.1
Mouzos & Makkai 2004	Australia	Interview	310	57	18.4
Pennell, et al. 2000	San Diego Co.	Prospective	2,756	536	19.4
Hester, et al. 2003	Three British Cities	Prospective	291	60	20.6
HMCPSI & HMIC 2004	Six British Jurisdictions	Prospective	118	25	21.2
Steketee, et al. 2000	Washington	Interview	247	54	21.9
Pennell & Burke 2002	San Diego Co.	Prospective	1,571	350	22.3
Ames 2001	Clinton Co.	Prospective	1,559	353	22.6
Hirschel, et al.1991	Charlotte	Prospective	686	156	22.7
Jaffe & Burris 1981	London	Prospective	444	104	23.4
Tjaden & Thoennes 2000E	U.S	Interview	713	176	24.7
Fagan 1989	Five Jurisdictions	Interview	270	74	27.4
Keilitz, et al. 1997	Three U.S. Jurisdictions	Prospective	285	82	28.8
Worden 2001	Five New York Towns	Prospective	2,129	631	29.6
Hester & Westmarland 2005	Cheshire; Tauton	Prospective	1,240	399	32.2
Lerman 1981	Westchester Co.	Prospective	1,218	416	34.2
Rauma 1984	Santa Barbara	Prospective	199	75	37.7
Steinman 1991	Lancaster Co.	Prospective	338	140	41.4
Lerman 1981	Seattle	Prospective	2,630	1,116	42.4
Schulman 1979	Kentucky	Interview	79	35	44.2
Tolman & Weisz 1995	Dupage Co.	Prospective	341	157	46.0
USCCR 1982	Maricopa Co.	Prospective	23	11	47.8
Wordes 2000	Berkeley	Prospective	138	66	47.8
Friday, et al. 2006	Charlotte	Prospective	891	439	49.3
Jaffe, et al. 1993	London	Prospective	90	52	57.8
Woolery 2004	Wisconsin	Snapshot	27,454	17,021	62.0
Urbis Keys Young 2001	ACT	Prospective	332	206	62.0
Jaffe, et al. 1986	London	Prospective	443	320	72.2
Jaffe, et al. 1993	London	Prospective	1,296	1,007	77.7
Brown 2004	Edmonton	Snapshot	2,934	2,317	79.0
Forty Studies with Offense and Prosecution Data			**79,146**	**28,104**	**35.5**
Patterson 2003	154 police agencies	Prospective	34,609	27,566	79.6
Trainor, et al. 2002	166 Canadian Agencies	Snapshot	30,806	25,192	81.8
Ogrodnik 2006	64 urban areas	Snapshot	211,791	177,904	84.0
Three Large Studies with Exceptionally High Rates			**277,206**	**230,662**	**83.2**
Forty Three Studies with Offense and Prosecution Data			**356,352**	**258,766**	**72.6**

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